





F. xvii. S.

20/



22101012676

Received by W. H. L. 72.6.

W. H. L.



Digitized by the Internet Archive  
in 2019 with funding from  
Wellcome Library

[https://archive.org/details/b31352716\\_0001](https://archive.org/details/b31352716_0001)



# YELLOW FEVER

An Epidemiological and Historical  
Study of Its Place of Origin





# YELLOW FEVER

## An Epidemiological and Historical Study of Its Place of Origin

BY

HENRY ROSE CARTER, M.D.

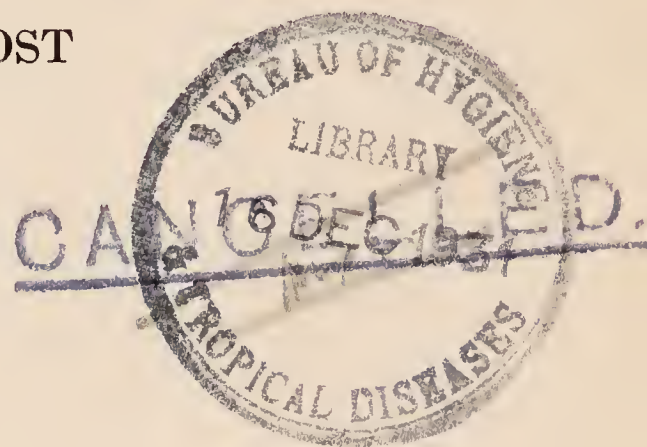
*Assistant Surgeon-General (retired)  
United States Public Health Service*

EDITED BY

LAURA ARMISTEAD CARTER

AND

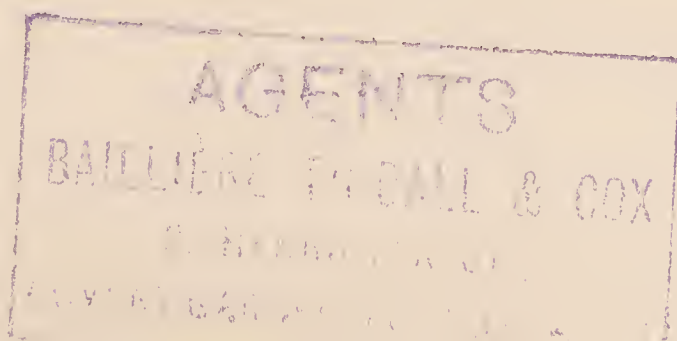
WADE HAMPTON FROST



BALTIMORE

THE WILLIAMS & WILKINS COMPANY

1931



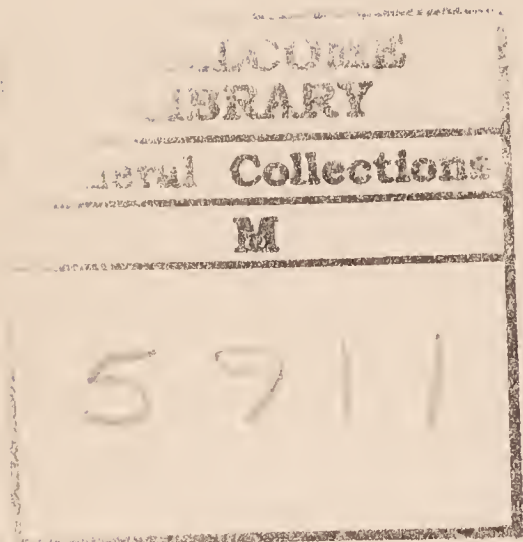
72315  
~~17298~~

COPYRIGHT 1931

THE WILLIAMS & WILKINS COMPANY

*Made in the United States of America*

Published August, 1931



COMPOSED AND PRINTED AT THE  
WAVERLY PRESS, INC.  
FOR  
THE WILLIAMS & WILKINS COMPANY,  
BALTIMORE, MD., U. S. A.



## AUTHOR'S PREFACE

It would seem natural, in writing a history of any disease, first to collect the different accounts of its occurrence and from them to determine its extent and severity in different epochs. From these data would be determined its extension or diminution in area and in intensity—time being our other variant. Ultimately, from the historical facts thus presented, the epidemiology of the disease would, or might, be derived. Thus, indeed, have our existing histories of diseases been written.

A different—almost a reversed—method will be followed for yellow fever in this work, and for these reasons: The reports and accounts of the occurrence of yellow fever—and of its absence as well—are, in a number of cases, not only unreliable, but erroneous. This was inevitable. The diagnosis of yellow fever is not always clear, and it was especially apt to be mistaken when yellow fever was considered, on the one hand, to be an intense form of malarial fever, and on the other, typhus fever modified by a hot climate. Both of these diseases have been reported as yellow fever, and yellow fever has been reported certainly as the first, if not as the second.

On the other hand, certain facts in the epidemiology of yellow fever are established and definitely known, and by them we can, in many cases, test—in some test absolutely—the truth or error of the account submitted to us. We know quite well the conditions necessary for the existence of yellow fever, for it to become epidemic and to continue at any place; and when we find its existence or an epidemic reported where the conditions necessary therefor do not exist, we know that the disease reported as such is not yellow fever. Similarly, when yellow fever is reported as continuing for a term of years at a place under conditions where this continuance is impossible, we know either that the disease in question was not yellow fever or that it ceased and was reintroduced.

Provided that we are conservative and postulate no conditions except such as are proven, there is no escape from these two conclusions; and by them not a few questions which have given rise to much and bitter controversy can be settled definitely and—what is more important—settled correctly.

It is for this reason that this work will begin with the statement of a few principles established in the epidemiology of yellow fever by which we may be able to sift some of the historical evidence to be submitted later.



## NOTE BY THE EDITORS

Early in 1922, shortly after his retirement from active duty in the United States Public Health Service, the author of this work, at the request of Dr. Wickliffe Rose, then Director of the International Health Board of the Rockefeller Foundation, and with material assistance from that organization, undertook the writing of a history of yellow fever. This was an undertaking which he had long contemplated whenever he should have the leisure, and for which he had prepared himself by a lifetime devoted very largely to the study of yellow fever in the field and in the literature.

As originally projected, the work was to have been a history of yellow fever to the present time, extending the histories by Bérenger-Féraud and others to include the period since the establishment of scientific and effective control, and reviewing earlier history in the light of modern knowledge. From the first, however, this plan was conditional upon his health and what he might consider a reasonable expectation of life, and it soon became apparent that this would not suffice for any work of more than a few years' duration. His effort was, therefore, directed to the completion, during his lifetime, of a section which would be complete in itself, which could be only the early history of the disease.

Even this was completed only by working throughout an illness severe enough to confine him to bed for long intervals, and, during his last year, continuously. At his death, in 1925, the work that is here presented had been fully drafted in its present scope and content. However, none of it had undergone final revision, and while the section on epidemiology and other portions which had been written quite early would probably have been revised but little, the latter part of the manuscript, dealing with Africa, had been drafted only roughly, with fairly numerous notes indicating more or less definitely contemplated revisions or additional references to be consulted.

In preparing the material for publication we have, of course, made such alterations as were clearly indicated by the author's notes, with some rearrangements in sequence and subdivisions. We have, however, refrained from any revisions excepting such as we are confident the author himself would have made. Consequently, the work is presented with considerably less revision than we think it would have received at his hands.

So many of the author's friends have contributed to his work in one way or another that it is hardly possible to make full acknowledgment. In his behalf we are happy to acknowledge indebtedness to Dr. Wickliffe Rose, Director of the International Health Board, and to his successor, General F. F. Russell, for indispensable assistance in many ways, and equally for their interest and encouragement; to Dr. Juan Martinez Hernandez of Merida, Yucatan, and Dr. Sylvanus G. Morley of the Carnegie Institution of Washington, for advice and information on Maya archaeology; to Dr. F. W. Edwards of the British Museum and Dr. L. O. Howard of the United States Bureau of Entomology, for assistance in matters entomological; to Dr. Joseph H. White of the United States Public Health Service and to Drs. Michael E. Connor, E. J. Scannell, G. Jameson Carr, and Henry Hanson of the staff of the International Health Board, for notes on various subjects; and to the librarians and staff of the Library of Congress, the Library of the Surgeon General of the Army, and the Public Health Service Library, for many special courtesies.

In the verification and listing of bibliographic references, we have received invaluable assistance from Miss Grace Brown, of the Catalogue Division of the Library of Congress, to whom we are especially indebted for this and many other services, as also to Mr. Charles Martel, Chief of the Catalogue Division of the same Library, for his assistance and advice.





## CONTENTS

### PART I. THE EPIDEMIOLOGY OF YELLOW FEVER

CHAPTER	PAGE
I. CONDITIONS REQUISITE FOR THE CONTINUED EXISTENCE OF YELLOW FEVER.....	3
The man sick of yellow fever. The insect vector. The man infected by the mosquito. General postulate. The first factor: the causative organism. The second factor: active <i>Aedes aegypti</i> mosquitoes, breeding places, temperature. The third factor: the man susceptible to yellow fever, mechanism of the spontaneous elimination of yellow fever by failure of the human host, critical number of <i>aegypti</i> , proportion of immunizations to supply of susceptibles, conditions of regional endemicity.	
II. DISPERSION AND GEOGRAPHICAL DISTRIBUTION OF AÆDES AEGYPTI.....	24
Mechanism of dispersion of <i>Aedes aegypti</i> . Requirements for permanent existence of <i>Aedes aegypti</i> : breeding places, temperature. Temperature limits of permanent life-zones of <i>Aedes aegypti</i> : biological zones, the two limiting conditions of temperature. Geographic distribution of <i>Aedes aegypti</i> in America.	
III. DISPERSION AND GEOGRAPHICAL DISTRIBUTION OF YELLOW FEVER...	34
Transportation of infected mosquitoes. Transportation of infected men. Combined method of dispersion. Time element in conveyance by vessels. Rate of spread on introduction. Reinfection of infecting places.	
IV. RELATION OF TEMPERATURE AND SOCIOLOGICAL CONDITIONS TO THE OCCURRENCE AND PERSISTENCE OF YELLOW FEVER.....	40
Infectible and non-infectible territory; relation to seasonal temperatures. Zones of occurrence of yellow fever: epidemic areas, endemic areas—temporary, permanent, and regional.	

### PART II. DISEASES WHICH HAVE, OR MIGHT HAVE, BEEN CONFUSED WITH YELLOW FEVER IN THE PAST

49

Distinctive characteristics of yellow fever. Diseases to be distinguished from yellow fever in older records: smallpox, measles, influenza, dysentery, plague, relapsing fever and spirochetal infections generally, typhus fever, scurvy, dengue, malarial fevers. Differentiation between yellow fever and malarial fevers: in epidemiology, general and special; in symptomatology. Sickness and mortality among early colonists in new countries: experiences in settlement of Jamestown and other North American colonies; the settlement of Martinique and Guadeloupe; summary.

## PART III. THE PLACE OF ORIGIN OF YELLOW FEVER

CHAPTER	PAGE
I. GENERAL CONSIDERATIONS.....	81
The regions to be considered. Biological evidence on place of origin of yellow fever; the reaction of the negro to yellow fever; evidence relating to the evolution and dispersion of <i>Aedes aegypti</i> . Historical evidence on place of origin of yellow fever. Regions for examination in America: the Gulf Coast of Mexico ( <i>tierra caliente</i> ); the Maya country of Yucatan and Central America; the coastal region of Peru; the Carib region.	
II. MEXICO: RECORDS.....	90
Mexican (native) records. Spanish writers of the first group, recording the history of the natives. Spanish writers of the second group, concerned with Spanish history in Mexico.	
III. MEXICO: TIERRA FRIA.....	96
Sickness and mortality before the Conquest. Sickness and mortality after the Conquest: various occurrences, 1520 to 1595; probable occurrence of typhus, 1526; the epidemics of 1544-45 and 1576 ("matlazahuatl"); scant records of sickness by Diaz del Castillo.	
IV. MEXICO: TIERRA CALIENTE.....	110
Herrera's account of unhealthfulness of the low country; ill repute of Vera Cruz; the tradition of "cocoliztle." Reputation of unhealthfulness from other sources. Sickness during occupation by Cortez.	
V. THE MAYA COUNTRY: CULTURE AND RECORDS OF PESTILENCES (MAYA CIMLAL AND XEKIK).....	116
Early Maya culture and records; inscriptions and manuscripts; chronology; the "Books of Chilan Balam" and other native chronicles. Native records of pestilences: "mayacimil" and smallpox. References in Maya records to "xekik" (blood-vomit); quotations and discussion of their significance. Descriptions of Maya pestilences by Europeans from Maya tradition.	
VI. THE MAYA COUNTRY: COMING OF THE SPANIARDS.....	136
Early expeditions. Montejo's expedition, 1527, and sickness therein. Negative record, 1528 to 1648. Question of the existence of yellow fever in Yucatan prior to 1648; possibilities to be considered; relation to question of existence of yellow fever in <i>tierra caliente</i> of Mexico.	
VII. THE MAYA COUNTRY: THE EPIDEMIC OF 1648 IN YUCATAN.....	145
The record of sickness up to 1648. López de Cogolludo's account of the epidemic of 1648. Possible origin of the epidemic, relation to activity of buccaneers.	
VIII. HAYTI (HISPANIOLA).....	154
Argument of Finlay, Bérenger-Féraud, and others. Sickness in Columbus' colony, 1493. Second epidemic, 1495. Subsequent history of Hispaniola.	



CHAPTER	PAGE
IX. THE ISTHMUS OF PANAMA: EXPEDITIONS OF OJEDA AND NICUESA . . . . .	159
Historical authorities: Herrera, Las Casas, Oviedo. Preparations for the expeditions. Ojeda's expedition: losses in fighting and from other causes. Nicuesa's expedition: Olano's treachery; hardships endured; mortality.	
X. THE ISTHMUS OF PANAMA: THE COLONY AT DARIEN . . . . .	174
Balboa; health of the colony under his administration. Pedrarias and the " <i>modorra</i> ;" accounts of the sickness and mortality; discussion. Pedrarias the Younger; sickness in his expedition.	
XI. GUADELOUPE: THE COUP DE BARRE, 1635 . . . . .	180
Authorities; settlement of Guadeloupe, 1627; famine; the " <i>coup de barre</i> " described by du Tertre and Breton; significance of the data; later description by du Tertre; summary.	
XII. YELLOW FEVER IN AMERICA AFTER 1648 . . . . .	187
Cuba: freedom from yellow fever before 1648; the epidemic of 1648 or 1649 in Havana; the malignant fevers of 1620; extension of yellow fever in Cuba, 1649-55; endemic yellow fever in Havana in later years. Other Caribbean regions and Brazil: frequent occurrence of yellow fever epidemics in West Indies after 1648; the epidemic of 1686 in Brazil; question of its source; the " <i>mal de Siam</i> ," 1690; early nomenclature.	
XIII. AFRICA: GENERAL CONSIDERATIONS . . . . .	198
The area to be examined: Senegal and Gambia; the Gulf of Guinea. Records; contrast with American records. Difficulties of recognition of yellow fever in Africa; small size of colonies; unrecognizable reaction in the negro; high prevalence of severe malaria. Criteria for identification of yellow fever in Africa; importance of island colonies and of histories of epidemics in fleets.	
XIV. WEST AFRICA: DISCOVERY AND SETTLEMENT . . . . .	208
Ancient knowledge of the West Coast. Alleged voyages of the Dieppois in fourteenth century. Early Portuguese discoveries, fifteenth century. Discovery of the islands off the West Coast. Beginnings of the slave trade. Early settlements on the mainland: Arguim, Elmina, Angola. Island settlements; size and importance.	
XV. EARLY RECORDS OF SICKNESS ON THE WEST AFRICAN COAST . . . . .	215
Sickness during discovery; rarity of recorded occurrence. Sickness in commercial voyages of Santarem and Escobar, 1471; Windham and Pinteado, 1553; Bird and Newton, 1588; Towerson; Lok, 1554; lack of definite evidence of yellow fever. Sickness in early settlements: Arguim, São Jorge da Mina, Angola, island colonies. Summary.	
XVI. THE ISLAND OF SÃO THOMÉ . . . . .	222
Sickness during settlement; failure of first attempt to colonize; forced settlement of <i>déportés</i> ; customary acclimation by residence in Angola. Growth of the colony; the "Portuguese Pilot's" account of the island. Sickness prevalent after settlement; accounts by the "Portuguese Pilot," Dapper, Barbot, and Labat. Indications of	



CHAPTER	PAGE
yellow fever on the island from the descriptions given of fevers and so-called " <i>bitios</i> ." Sickness in Dutch fleets at São Thomé, 1599 and 1641; question of yellow fever.	
XVII. THE CAPE VERDE ISLANDS.....	231
Discovery and settlement. Relation to African slave-trade. São Thiago, a port of call. Unhealthfulness of São Thiago. Expedition of Drake and Carleill, 1585; landing at São Thiago; epidemic at sea and in the West Indies. Sickness in expeditions of Menezes, 1624, and Mascarenhas, 1638-39, and alleged epidemic in Brazil, 1640.	
XVIII. THE CANARY ISLANDS AND THE EPIDEMIC IN VAN DER DOES' EXPEDITION, 1599.....	241
Discovery and settlement; epidemic during conquest of Teneriffe, 1495. Trade and communications; special liability to infection from Africa, 1581 to 1640. The expedition of Van der Does, 1599; Hakluyt's account; Pop's account of epidemic at sea; accounts by Dapper, Barbot, and Labat of epidemic after arrival at São Thomé; questions concerning the nature of the sickness, en route from the Canaries to São Thomé, on the island of São Thomé, and en route thence to Holland. Editors' note: supplementary data.	
XIX. SICKNESS IN AFRICA CONSIDERED TO BE CERTAINLY YELLOW FEVER.....	254
Yellow fever recognized later in Africa than in America. Lind's descriptions of possible yellow fever, 1768. Schotte's description of the epidemic of 1778 in Senegal, epidemiological and clinical description, differential diagnosis. Yellow fever, when first described in Africa, not considered a "new disease." Occurrence of yellow fever in Africa after 1778; reports refer only to occurrence in European settlements or aboard ships; reasons for belief that yellow fever is of constant occurrence, in unrecognized form, in West African negroes.	
XX. THE PLACE OF ORIGIN OF YELLOW FEVER: SUMMARY.....	265

PART I

THE EPIDEMIOLOGY OF YELLOW FEVER





## CHAPTER I

### CONDITIONS REQUISITE FOR THE CONTINUED EXISTENCE OF YELLOW FEVER

*Yellow fever is contracted by man in nature from the bite of a mosquito, Aedes (Stegomyia) aegypti (Linnaeus),<sup>1,2</sup> itself infected by having fed on a man sick of that disease, and, so far as known, is in nature only thus contracted.*

#### (A) THE MAN SICK OF YELLOW FEVER

- (1) He is infective to the insect in the first few days of the disease.
- (2) There is evidence that he is not so infective long. None have been shown to be so after the third day, but the limit of time has not been established.

#### (B) THE INSECT VECTOR

- (1) So far as known, only the insect named acts as a vector of this disease.<sup>3</sup>

(2) So far as known, the only source of infection to the insect, of sanitary importance in nature, is the man sick of yellow fever.

(3) A certain time elapses from the date the mosquito feeds on the sick man before she can communicate the infection. This interval, which is called *the period of extrinsic incubation of yellow fever*, is usually stated as being not less than twelve days, but its minimal limit is not definitely determined. In experimental cases no mosquito has proved infective to man which had not fed on a man with yellow fever at least twelve days previously.<sup>3</sup>

<sup>1</sup> Determination of species made by Linnaeus himself, 1762. (Dyar, 1920. See also letter from Edwards, quoted by Dyar.)

The writer hesitated long whether to use *Stegomyia* as the vernacular form for the name of this vector, simply because he and other medical writers had always done so. For the Americas this has been allowable and definitive, because no other species of this subgenus is found here. For a general history, however, as there are a number of *Aedes (Stegomyia)* species in the Old World, this would not be definitive of the only known vector, and for this reason he decided to use the species name, which is definitive everywhere.

<sup>2</sup> For identification of bibliographic references, see alphabetical list of authors, pp. 272-289.

<sup>3</sup> See Editors' note, p. 6.

In one of the cases observed by the writer at Orwood, Mississippi, in 1898 (Carter, H. R., 1900-a and 1901-b)—which observations formed the basis of the doctrine of extrinsic incubation—the interval between the occurrence of the infecting and a secondary case was recorded as eleven and one-half days, which would give eight and one-half days as the maximum period of *extrinsic* incubation. Naturally, the dates of the *occurrence* of cases are less exactly recorded from histories given by laymen (patients and members of their families), even if much pains be taken, than in experimental cases. The *direction* of error, however, in the former would be rather to lengthen this apparent interval than to shorten it.

(4) Once infective, the insect remains so as long as we have observed her—probably all her life. A feeding fifty-seven days<sup>4</sup> after the infecting feeding has proved positive (Reed, Carroll, and Agramonte, 1901).

(5) The evidence is decidedly against the transmission of infection to her progeny—certainly not so as to be of general sanitary importance.<sup>4</sup> A single case, the nature of which was not as definitely determined as we could wish, is reported by the French Commission working at Rio (Marchoux and Simond, 1906). Many negative results were obtained by Working Party No. 3 of the U. S. Public Health and Marine-Hospital Service at Vera Cruz (Rosenau and Goldberger, 1906) in 1903, and, incidentally, the same result was obtained in 1901 and 1902 at Las Animas Hospital, Havana, where, among the mosquitoes kept over from Reed's experiments for use the next year, were a number which were the progeny of those that had conveyed yellow fever in 1900. These, so long as they lived, were fed from the hands of people immune or susceptible to yellow fever indiscriminately, without anyone's showing infection.

(6) The mosquito, both before and after becoming infective, feeds by day and by night (see Reed and Carroll, 1901; Howard, Dyar, and Knab, 1912-17, vol. 1, pp. 262-270; Macfie, 1915; Seidelin and Summers-Connall, 1914; Gordon and Young, 1921; and Carter, 1906).

#### (C) THE MAN INFECTED BY THE MOSQUITO

(1) He must be susceptible to yellow fever.

(2) The period of incubation for yellow fever in man, that is, the time from the date of infection to the beginning of the attack, is, in nature, roughly from three to six days.

<sup>4</sup> See Editors' note, p. 6.



Of forty-four cases observed—thirty-two experimental (various observers) and twelve single short exposures (Carter, 1901-a)—the minimum, one case only, was two hours less than three days; the maximum, one case only, two hours more than six days.

(3) After the attack he is, in general, immune to yellow fever, to the infection as well as to the disease. The writer believes this immunity to be as permanent as that conferred by other strongly immunizing infections.<sup>5</sup> That he is immune for a time is established.

(4) Besides man and the *Aedes aegypti* mosquito, we know of no third host for the causative organism of yellow fever which is, in nature, of sanitary importance, nor of any free-living state of existence of the organism.<sup>6</sup>

<sup>5</sup> See Editors' note, p. 7.

<sup>6</sup> Note by the Editors:

#### RECENT EXPERIMENTAL STUDIES OF YELLOW FEVER

Since this manuscript was written, and subsequent to the author's death, experimental studies have added much to previous knowledge of the virus of yellow fever, the mechanism of its transmission, and the pathology and immunology of the disease. Beginning with the discovery, in 1927, by Stokes, Bauer, and Hudson (1), of the West African Yellow Fever Commission of the Rockefeller Foundation, that Indian monkeys (*Macacus rhesus* and *Macacus sinicus*) were readily susceptible to this infection, experimental investigations have developed so rapidly that within three years the literature has already become too extensive for review in a brief space. Moreover, work in this field is still in a very active stage, so that a review which would be complete at this time (September, 1930) might be wholly inadequate within a few months. Therefore, comment here is limited to noting recent observations on points to which Dr. Carter specifically refers in his discussion of the epidemiology of the disease, with no attempt to note all recent contributions of importance.

(1) *Susceptible animals.* Several species of Indian monkeys (*Macacus rhesus*, *M. sinicus*, and others) have been found susceptible to experimental infection, developing a reaction which is well marked and frequently fatal, characterized by gross and microscopic lesions which definitely identify it with yellow fever in man, and resulting in immunity which is demonstrable by resistance to subsequent inoculation and by protective properties of the blood-serum. A number of Brazilian monkeys (species of *Cebus*, *Lagothrix*, *Atelus*, and *Saimiri*) have been found more or less susceptible, showing evidence of multiplication of the virus and development of immune bodies, but usually without severe reaction or distinctive lesions (2, 3, 4). Chimpanzees (African) and such varieties of African monkeys as have been tested have proved insusceptible to the disease but, in some instances, have been found capable of transmitting the infection some days after inoculation and have shown evidence of the development of protective antibodies (1, 19). Mice have been shown to be susceptible to a specific effect of the virus when it is introduced directly into the central nervous system (20).

(2) *Sources and modes of infection.* Susceptible animals have been successfully



## GENERAL POSTULATE

From the facts stated it is evident that, in general, we have the postulate: *For the naturally continued existence of yellow fever in any place there are necessary three factors, coexistent at that place:*

- (1) *The causative organism of the disease.*
- (2) *Functionally active Aedes (Stegomyia) aegypti mosquitoes.* Pos-

infected by direct inoculation of blood from infective men (1); by inoculation of blood and tissues from infective monkeys; by the bites of *Aedes aegypti* previously fed on infective monkeys—after a period of extrinsic incubation—and by the injection of macerated bodies or parts of bodies of such mosquitoes within the period of extrinsic incubation (5, 6, 7).

The blood of persons suffering from yellow fever has been found infective for monkeys within the first three days of illness but not thereafter (8, 9), confirming the observations in earlier human experiments. In monkeys infected by mosquitoes, the blood has been found infective through mosquitoes 24 hours after inoculation, that is, well before the onset of a distinct febrile reaction (10).

*Aedes aegypti* have been found infective to monkeys, by biting, as early as nine days after first feeding on an infected monkey (10). This is three days less than the minimum period of extrinsic incubation observed in mosquitoes infected by biting man.

A single *Aedes aegypti* has been found infective for monkeys certainly 91 days and probably 101 days after last infective feeding (1). Infectivity, once established in the mosquito, appears to persist throughout the life of the insect (1).

The possibility of transmission of infectivity from infective female mosquitoes to their progeny has been extensively investigated, with uniformly negative results (1, 11).

(3) *Vectors other than Aedes aegypti.* In addition to *Aedes (Stegomyia) aegypti*, several other species of mosquitoes have been found capable of transmitting the infection from monkey to monkey by their bites after a period of extrinsic incubation, namely: of African mosquitoes, *Aedes (Stegomyia) luteocephalus* (Newst); *Aedes (Aedimorphus) apicoannulatus* (Edw); *Aedes vittatus* (Bigot) (= *sugens* Theo.); *Aedes africanus* (Theo.); *Aedes simpsoni* (Theo.); *Eretmopodites chrysogaster* (Graham); and *Taeniorhyncus (Mansonoides) africanus* (Theo.); of American mosquitoes, *Aedes (Ochlerotatus) scapularis*. Infection, not by biting, but by injection of macerated bodies, has been obtained with *Anopheles gambiae* (Giles) (= *Costalis* Lw.) in Africa; and with *Aedes (Ochlerotatus) serratus* and *Aedes (Taeniorhyncus) taeniorhyncus* in America. How far any of these, other than *Aedes aegypti*, may be factors of sanitary importance in the natural transmission of the infection is at present undetermined (12, 13, 14, 15).

(4) *Immunological identity of West African and American virus.* So far as they have been compared, strains of yellow fever virus from West Africa and Brazil, though differing in virulence for monkeys, have been found immunologically the same (16, 17).

(5) *Mild reaction in the negro.* Strains of virus of high virulence for monkeys have been isolated from West African negroes suffering from relatively mild ill-

sibly some other species of this subgenus may prove also to be a vector—*Aedes* (*Stegomyia*) *alba picta* is very similar in its biology to *aegypti*. However, *aegypti* is the only species of this subgenus found in the Americas and, owing to the invariable disappearance of yellow fever when this species—and this species alone—is sufficiently controlled, it seems quite certain that in the Americas no other mosquito associated closely with man is a vector.

(3) *Men susceptible to the disease—the mosquitoes having access to both of the other two.*

---

ness, which, at least in one instance, could not have been diagnosed with certainty as yellow fever from clinical evidence alone (1, 18).

(6) *Duration of immunity.* Protective properties against the virus have been demonstrated in the serum of persons who had recovered from yellow fever as long as 26 years previously.

(7) *Specific immunity in endemic areas.* Protective properties against the virus have been demonstrated in the serum of a large number of persons not known to have suffered from yellow fever at any time, but only in persons resident in areas of West Africa where yellow fever is known or legitimately suspected to have been prevalent within their lifetime (21). This confirms the epidemiological evidence that, in West Africa, yellow fever prevails among negroes as an unrecognized and perhaps clinically unrecognizable infection.

(8) *Relation of yellow fever to leptospiral jaundice.* Yellow fever is clearly shown to be, in the nature of its virus, and in its pathology, quite distinct from the infection caused by *Leptospira icteroides*, although leptospiral jaundice in man may be clinically almost indistinguishable from yellow fever (17, and others).

Since these and other observations made since 1925 in no way conflict with the view which Dr. Carter presents of the epidemiology of yellow fever, but tend, in general, to confirm inferences or conjectures previously lacking experimental proof, his discussion is presented without change, but with notes here and there calling attention to necessary modifications in certain statements of detail rather than principle. It is true that when Dr. Carter's discussion was written he was inclined to accept, though not without reservation, the evidence which seemed, at that time, to indicate *Leptospira icteroides* as the specific cause of yellow fever. This, however, in no way modified his views of the fundamental epidemiology of the disease.

#### References

1. STOKES, A., BAUER, J. H., AND HUDSON, N. P., 1928, Am. Jour. Trop. Med., **8**: 103.
2. DAVIS, N. C., AND SHANNON, R. C., 1929, Jour. Exper. Med., **50**: 81.
3. DAVIS, N. C., 1930, Am. Jour. Hyg., **11**: 321.
4. ——— 1930, Jour. Exper. Med., **51**: 703.
5. HINDLE, EDW., 1929, Trans. Royal Soc. Trop. Med. and Hyg., **22**: 405.
6. ARAGÃO, H. DEB., 1929, Brazil-Medico, **43**: 843.
7. DAVIS, N. C., AND SHANNON, R. C., 1930, Am. Jour. Hyg., **11**: 335.
8. BAUER, J. H., AND HUDSON, N. P., 1928, Am. Jour. Trop. Med., **8**: 371.



The causative organism may be (a) in men affected with yellow fever,<sup>7</sup> or (b) in the mosquitoes themselves.

Possibly one should add to the above: (4) "A temperature sufficiently high to allow the necessary development of the causative organism in the mosquito." Certainly this development is necessary; and it is possible that the temperature required for it is in excess of the minimum required for the functional activity of *aegypti*. This has not been proved, however, and one is unwilling to introduce an extra condition as a necessary one unless it clearly is essential, and this one may be already implicitly provided for in the second condition—the temperature necessary for the functional activity of *aegypti* being sufficient for the development of the causative organism in the mosquito.

#### THE FIRST FACTOR: THE CAUSATIVE ORGANISM

The causative organism will, in nature, continue to exist in the place in question only if both the other factors coexist there with it, that is, if there be active *aegypti* to convey it to susceptible men, in whom its life is continued and from whom other *aegypti* obtain it to transmit again to other men. If either of these two factors, susceptible men or func-

9. ARAGÃO, H. DE B., 1928, Inst. Oswaldo Cruz, Suppl. das Memorias, No. 2.
10. HUDSON, N. P., AND PHILIP, C. B., 1929, Jour. Exper. Med., **50**: 583.
11. PHILIP, C. B., 1929, Jour. Exper. Med., **50**: 703.
12. BAUER, J. H., 1928, Am. Jour. Trop. Med., **8**: 261.
13. PHILIP, C. B., 1929, Am. Jour. Trop. Med., **9**: 267.
14. ——— 1930, *ibid.*, **10**: 1.
15. DAVIS, N. C., AND SHANNON, R. C., 1929, Jour. Exper. Med., **50**: 803.
16. BAUER, J. H., AND HUDSON, N. P., 1930, Jour. Prev. Med., **4**: 177.
17. SAWYER, W. A., KITCHEN, S. F., FROBISHER, M., AND LLOYD, W., 1930, Jour. Exper. Med., **51**: 493.
18. MATHIS, C., SELLARDS, A. W., AND LAIGRET, V., 1928, Compt. rend. Acad. de Sc., **186**: 604.
19. BAUER, J. H., AND MAHAFFY, A. F., 1930, Am. Jour. Hyg., **12**: 155.
20. THEILER, MAX, 1930, Ann. Trop. Med. and Parasitol., **24**: 247.
21. BEEUWKES, H., BAUER, J. H., AND MAHAFFY, A. F., 1930, Am. Jour. Trop. Med., **10**: 305.

<sup>7</sup> Or in some other animal infective to *A. aegypti*. The common occurrence of the "elimination of yellow fever by failure of the human host" in the Americas is strong evidence that there is no such animal intimately associated with man in the Western Hemisphere.



tionally active *aegypti*, fails for a sufficient time, the micro-organism disappears and the disease will not reappear unless the micro-organism be reintroduced, even if the other two factors have reappeared in the meantime.

The limit of time necessary for the elimination of the micro-organism by the absence of active *aegypti* or of susceptible men is the time the micro-organism can live in the host it is occupying without transference.

If it were *aegypti* which were lacking, and the parasite existed only in men (or some other animal infective to *A. aegypti*), this time would be so much of the period of incubation as had not yet elapsed, plus the time the man developing yellow fever was infective to mosquitoes—the sum certainly not in excess of two weeks, probably not over ten or eleven days. Nine days is about the sum of these maxima as they have been observed so far.

If the causative organism existed only in the mosquito, and it was the man susceptible to yellow fever who was lacking, the potential life of the micro-organism would be much longer. So far as we know, the insect, once infected, is infective as long as she lives and is active. The longest time recorded is, I think, fifty-seven days<sup>8</sup> in one of the Quemados cases (Reed, Carroll, and Agramonte, 1901).

Although an infected female mosquito has been kept alive five months in the laboratory, there is evidence that in nature mosquitoes cease to infect men—probably die—within two months of the destruction of their breeding places (Carter, H. R., 1922). It would seem, then, that three months would cover the ordinary limit in nature of the life of the parasite in the insect without change of host. Nevertheless, in the epidemiology of yellow fever the possibility of the occasional survival of *aegypti* in nature longer than the ordinary limit, or even as long as they have lived under laboratory conditions, must always be considered. Such insects, if infected, might give rise to cases of yellow fever long after the breeding places of the mosquito had been eliminated.

Note here that the limits of time given as sufficient for the elimination of the causative organism are believed to be correct and conservative, yet it is not claimed that they are “established” in the same sense as the postulate which has been stated.

<sup>8</sup> See Editors' note, p. 6.

THE SECOND FACTOR: ACTIVE *AËDES AEGYPTI* MOSQUITOES

By "active" is meant functioning biologically, capable of flight, feeding and propagating normally.<sup>9</sup>

There are two conditions necessary for the continued existence of active *aegypti*: (1) suitable breeding places and (2) suitable temperatures.

*Breeding places.* *Aegypti* breed in collections of water in artificial—man-made—containers, and in containers similar to them, close to, or in, the habitations of men. Their preference is for small or moderately sized collections of clear water, especially for wooden vessels or calabashes, in shady and somewhat dark places.

As to the material of the container, Dr. G. Jameson Carr, working at Parahyba, Brazil (unpublished report to International Health Board, 1924), found that oviposition was decidedly greater in wooden containers than in either earthenware or metal, and greater in earthenware than in metal.<sup>10</sup> In some observations made by the writer, calabashes were preferred to wood.

*Aegypti* are, in the writer's opinion, and in that of the inspectors with whom he has worked, more often found in small collections of water than in large ones. This may not be a question of size, however. Small containers are more apt to be set in corners, and less apt to have been dis-

<sup>9</sup> Of course, if imagoes were continually introduced into a place where the conditions were such that they were capable of flight and feeding, yellow fever could be kept up by them even if there was no propagation. This, while possible, is a contingency that needs scarcely to be considered epidemiologically, as the cases in which the introduction of imagoes took place so continuously as to keep up the infection would be extremely rare. It might conceivably happen to some place just a little colder than Orizaba, in which the imago could feed, but not propagate, and in daily communication by train or vessel with a town infested with *aegypti*—especially if infected with yellow fever as well. I have no knowledge that this has ever occurred.

<sup>10</sup> One might naturally expect to find that the hydrogen-ion concentration would greatly influence the choice of water for oviposition, indeed that this might be the determining factor of the degree of preference shown for water-containers of different materials, the nature of the containers influencing the degree of concentration. Such investigation as has been made, however, does not seem to sustain this view. The limits of hydrogen-ion concentration reported compatible with free breeding are wide, and the writer is inclined to think that this factor is not of great importance in the waters otherwise suitable for breeding, as they occur in nature. The free breeding of *aegypti* in the *agua ceniza* of the laundresses may imply a preference for alkaline waters; and the fact too that, in the writer's experience, breeding is not common in rusty iron containers, the water in which should be of the opposite reaction, points in the same direction.



covered and disturbed in the preceding inspection. Yet *aegypti* have no objection to large bodies of water, being common, and occasionally abundant, in cisterns, etc. Indeed, as profuse breeding as the writer has seen, was in a cemented basement in a warehouse in Bahia, about 20 x 35 feet, and a little less than 2 feet deep. The top was covered with loose boards with cracks between them.

Since the larvae of this species are essentially bottom feeders, one would expect that they would prefer rather shallow water (if the ovipositing imago can determine the depth) and would, in any case, do better in it. The writer knows of no comparative tests on this matter.

The writer has never seen breeding at a distance from human habitations, and, so far as he can recall, only a single instance of it has been reported (MacGregor, 1919).

The preference of *aegypti* for artificial containers and those similar to them—from a practical standpoint, one might almost say their limitation to them—is too well established and conceded to require discussion. It is, I think, the universal finding of all who have carefully investigated the subject. I will take note of only one thing in this connection. Francis (1907), working at Mobile, states that “they were not found breeding in any” (collection of water with) “natural earth bottoms.” The context shows that he meant puddles in the earth, “mud puddles”; and this observation, I am quite sure, is correct. It is, however, the nature of the *sides* of the container *at the surface of the water*, not of the bottom, which seems to determine the suitability for *aegypti* breeding.

Slightly modifying Francis' statement, Dr. Henry Hanson (unpublished paper), on the basis of his Peruvian experience, says,<sup>11</sup> “we have” (in the Americas) “found them” (in nature) “breeding” (from oviposition to imago) “in no collection of water with” (exclusively) “earth” (i. e., mud) “sides” (at the surface of the water). This is believed to be absolutely correct.<sup>12</sup> Certainly this has been our experience in Havana,

<sup>11</sup> The words in parentheses are added to Hanson's statement of the formula; but he evidently intended it as it is here more fully expressed.

<sup>12</sup> The words “in the Americas” are introduced because the observations on which this statement is based were confined to the Americas. Recent observations (reports sent from the British Colonial Office to the International Health Board, August 10, 1923) state that at Salt Pond, Gold Coast, West Africa, this mosquito breeds freely, and even profusely, in mud holes and the tracks of children in the mud around the Lagoon and in the Lagoon itself, when artificial containers are not available. Breeding of this mosquito in the same class of places is implied, but only implied, in several official reports from this region, and apparently with artificial containers available. This matter is now under investigation (personal communication from J. W. W. Stevens, 1924). Boyce (1910) states



wherever we have studied breeding habits in the United States, on the Isthmus, in Guayaquil, and in Peru.<sup>13</sup> In the latter country *aegypti* were commonly found breeding freely in the large *botijas* (earthenware jars about 5 feet high) with from 2 to 6 inches of pretty solid mud at the bottom—the sediment resulting from many fillings with muddy water; and in many shallow wells, the sides of which were of wood or brick. In wells with sides altogether of earth the larvae of other mosquitoes were found, but *aegypti* never.

When we consider the method of oviposition, the insect standing at the very edge of the water, it seems reasonable that the nature of the side *at the surface of the water*, rather than the bottom of the container, should influence her in the choice of a breeding place. Certainly this has been the case in my experience.

The occasional occurrence of *aegypti* larvae in street gutters and even

---

that he has seen breeding of *aegypti* in pools on the mud tops of native houses in Africa. This observation is, I think, unique. They do breed in knot-holes in trees, hollows in rocks, etc., and in the water held in certain plants. These places are physically like their man-made breeding places. The last has, in my experience, been used only when the usual breeding places were not available.

At Colon our inspectors, for a time in the spring of 1905, thought they had discovered breeding in crab holes, but the striped mosquitoes breeding in these holes were *Deinocerites caneronum*, not *aegypti*. Seeing how very important this question is for control work in Africa, further investigation seems advisable.

<sup>13</sup> The matter has been further investigated quite recently, and some fifty reports of different men, mainly of the International Health Board staff, engaged in anti-*Stegomyia* work in different parts of Mexico, Central America, Colombia, the Guianas, and many parts of Brazil, state that the authors found no *Stegomyia* breeding in nature from oviposition to imago in "any collection of water, all the sides of which at the water's level are of mud." Carr at Parahyba, however, was able, by confining the insects in a cage of wire netting, to get oviposition well above the surface of the water in a cavity in sand, where the sand was wet by capillarity, the larvae hatching and crawling down into the water (unpublished report to International Health Board, Rockefeller Foundation, by Dr. G. Jameson Carr, 1924).

Breeding is reported, too, as common in the small collections of water in cocoanut palms on the east coast of Africa (Haworth, 1924) and in the axils of leaves of *Dracaena* and other plants in Sierra Leone, etc., by the Yellow Fever Commission (West African) (1915). Breeding in the axils of the leaves of certain plants, as in knot-holes in trees, and in holes in rocks, was common enough on the Isthmus, although not enough imagoes were so produced that we regarded it of sanitary importance: the first, too, only when water in artificial containers was not available. This class of container, however, is that which the writer (Carter, 1924-a) proposed to include as "similar" to artificial containers.

pools in the street, has been noted (LePrince and Orenstein, 1916, pp. 55 and 240; Carter, 1924-a; and others), but usually under conditions which have led the observer to believe that the larvae had been washed down into the place where found, from roof gutters and other such places which were, in effect, artificial containers. In no case were these larvae in sufficient numbers to be of sanitary importance.<sup>14</sup>

The presence of puddles in the streets, or ponds and marshy places in vacant lots, and the absence of sufficient sewerage and drainage in a town, all of which have been mentioned as very insanitary from the standpoint of yellow fever production,<sup>15</sup> have no effect whatever on the production of *aegypti*.

On the other hand, the storage of drinking water about the houses, in cisterns, jars, or any other receptacle, does favor their breeding markedly. In towns with no water supply in pipes, or with an insufficient one, where the people depend on conserving rain-water or the purchase of water from itinerant vendors, water must be stored in containers about the houses, and such furnish ideal breeding places for *aegypti*. Many a town has been freed of *aegypti* (and of yellow fever) by the introduction of a piped water supply, and the consequent doing away with the domestic storage of water. The need of storage is greater as the difficulty of obtaining water increases; thus it happens that the very scarcity of water—generally supposed to be a protection against all mosquitoes—inevitably leads to the production of *aegypti* in the towns and villages of such a country.

For the water in which they breed, we would say that the preference of *aegypti* is for clear, not necessarily clean, water. They are fairly adaptable about this, however, and will breed in muddy water, and even in somewhat brackish water. They breed freely in water to which

<sup>14</sup> The occurrence of *aegypti* larvae in nearly 80 per cent of the gutters examined on the outskirts of New Orleans, reported by the Mosquito Commission of New Orleans (Beyer, Pothier, *et al.*, 1902) is possibly referred to in the explanation given by Howard, Dyar, and Knab (1912-17, vol. i, p. 284): "It would seem that in these cases the larvae were not bred and most probably they were those of some other species of *Aedes*, many of which greatly resemble *calopus*" (*aegypti*) "in general appearance."

It must be noted, however, that many of the street gutters of New Orleans have their sides lined with cypress boards. Otherwise this finding of the writers cited above would be most unusual.

<sup>15</sup> Boyce (1911, p. 26): "Puddles formed everywhere, for there was no proper drainage. . . . These conditions would favor no doubt the propagation of the *Stegomyia*."



wood ashes have been added—the *agua ceniza* of the laundresses of Spanish America.<sup>16</sup> On the other hand, they do not breed in soapy water or in cesspools, many of which have been examined by the writer and his correspondents.

*Temperature.* The limits of temperature for the functional activity—breeding and feeding—of this mosquito are usually given from 68° to 102.5°F. The optimum is from 80° to 87°F. The lower limit is set by that necessary for *breeding*, since they feed—and may be able to transmit yellow fever—at a temperature below the inferior limit (68°F.) set for breeding.<sup>17</sup> The Pasteur Commission (Marchoux, Salimbeni, and Simond, 1903), working at Rio, found 64°F. (18°C.) as the lower limit

<sup>16</sup> The control of breeding in the *agua ceniza* has been a troublesome problem. It is too valuable to be thrown out. Oil and other larvicides render it worthless, and any attempt to get rid of it or lessen its value to the laundress leads to its concealment. In northern Peru, in 1920 and 1921, the writer controlled the production of imagoes in the *agua ceniza* by adding soap to it. This, of course, was not objected to and it was entirely successful. A saturated alcoholic solution of soap was used, but it was the soap that killed the larvae, not the alcohol. It has since been used with success in Mexico, Yucatan, and Brazil. Soap as a larvicide is mentioned by Howard, Dyar, and Knab (*loc. cit.*, vol. i, pp. 290–291).

<sup>17</sup> It has been suggested that the parasites in the insect host may perhaps be killed or rendered inactive by a fall of temperature to a point well above the minimum necessary for the functional activity of the mosquito. Data, epidemiological or experimental, are lacking on this subject, yet they might not be difficult to obtain. The analogy with malaria and Rocky Mountain fever, for which a similar thing is proven, is suggestive. The prolongation of the development of infectivity in *aegypti* as the temperature is lowered, while still high enough for the mosquito's functional activity, also seems to imply that a higher temperature is required for the development (or it may be for the existence) of the causative organism in the vector than for that of the insect itself.

It may be, then, that there are border-line places and times in which the insect can function, but in which it is too cold for the causative organism to develop in her. Thus, in Mexico City, from *aegypti* larvae received July 24 and August 8 from the low country, Fernando Lopez (1905) developed imagoes which were functionally active—flying, feeding on man, and laying eggs from which other imagoes were developed. These, however, did not oviposit, although they lived to December 1 and fed. Connor, too, reports the presence of the insect in Mexico City (personal communication, 1922, from Dr. M. E. Connor). Yet with the many introductions of men infected with yellow fever into Mexico City (and developing it there) we have no report of its having been propagated from them. The reason may have been that the temperature was too low for the development of the causative organism in the mosquito, although high enough for the functional activity of the insect. Certainly the “*tierra fria*” of Mexico is historically uninfectible territory, although functional activity of *aegypti* may sometimes be possible.



of temperature for feeding; Reed's Commission (Reed and Carroll, 1901) at Quemados placed it at 62°F.; Hanson, in Peru (personal communication from Dr. Henry Hanson, 1921), found *aegypti* "active and feeding in the evenings" at 17°C. (62.6°F.) in nature. Similarly, for the upper limit, Marchoux, Salimbeni, and Simond (1903) found heat above 39°C. (102.2°F.) fatal to imagoes. Macfie (1920), applying heat for five minutes only, and with abundant moisture, found them "fairly active at 39°C.; profoundly affected, but not completely inactivated at 40°C." (104°F.). They were more susceptible to heat if abundant moisture was not supplied. It would seem fair, then, to set the limits of temperature for such functional activity as would be of sanitary importance, that is, as would continue the existence of yellow fever under ordinary circumstances, at from about 72°F. to 101° or 101.5°F.

Obviously, it is implied that these temperatures are the ones to which the insects themselves are exposed—which may be very different from outside temperatures—and for some time.<sup>18</sup> A drop below 72°F. or even lower for a few days would not eliminate *aegypti*, nor would a temporary rise to 102°F. A rise above 102.5°F., to which they were exposed for some time, would probably kill off the majority of the imagoes. With the upper limit, however, we are not, in practice, much concerned (see page 40, footnote 1); but as to the lower limit, it is safe to say that an outside temperature of about 70° to 72°F., long continued, would be incompatible with the existence of yellow fever; especially in "yellow fever" countries, in which the houses are open and fires, except for cooking, are a rarity, *aegypti* would not breed at this temperature in sufficient number to continue the fever for long.<sup>19</sup>

<sup>18</sup> LePrince has recently reported (personal communications from Mr. J. A. LePrince, January 21 and March 20, 1922) finding *aegypti* reproducing and feeding in a hotel at Carbondale, Illinois, on January 19 and March 15, 1922. They "were breeding in the writing-room in one of the containers in which flower-pots were set." The outdoor temperature at this place must have been as low as 10°F. or 5°F. at times during the winter. Winter breeding of *aegypti*, even in well-warmed houses, is, however, altogether exceptional. The writer knows of no similar case.

<sup>19</sup> Finlay (1912) states that of imagoes developed near the lower temperature limit of breeding the proportion of males to females is about five to one; the proportion in hot weather is reversed. I have been unable to verify this, but, if true, the chance of continuing the fever is still further lessened near the inferior temperature limit of breeding by this as well as by slower oviposition and a much longer cycle of development from oviposition to the imago. Cavassa (1922), of Guadalupe, Peru, also finds males developed in excess in cool, and females in warm, weather. Further observation is needed before this can be considered established as a generalization.

Mosquitoes already infective might continue to convey yellow fever as long as they fed, as has often occurred among families returning to town "after the end of the epidemic." If, under these circumstances, an insect received infection from a man thus infected with yellow fever, she might even become infective and convey the disease. The period of extrinsic incubation, however, would be extremely long, and in the towns in the United States where this would be most apt to occur, the increasing cold would probably kill the mosquito before she became infective. Secondary cases in these returning families must be of extreme rarity. I do not know that any have been reported.

The imago dies quickly at freezing point. Flu (1920) states that at or below 6°C. (42.8°F.) none lived twenty-four hours. At 7° to 10°C. (44.6° to 50°F.) they are "immediately benumbed, but not past revival until four days afterwards." The larvae, in nature, are somewhat more resistant than the imago. This is indicated by observations in nature rather than by experiment; thus, Francis (1907) found larvae during every month at Mobile, although there was an occasional formation of ice in shallow water, and no imagoes were observed in winter.

The insect hibernates—where it does hibernate—in the egg form. The eggs are laid above water, or at the very edge of the water, and left on the sides of the container as the water goes down. Dried, they may retain their viability a long time; Francis (1907) has found them to remain viable for six and one-half months, and Bacot (1918), for over eight months. MacGregor (1915) reported some still viable after having been carried from the west coast of Africa to England, adhering to dried leaves. In this stage the insects are more resistant to low temperature than in the larval or imago stage. The eggs will hatch after freezing a short time; Reed and Carroll (1901), indeed, hatched a number after exposing them an hour to -17°C. (1.4°F.). Prolonged low temperature, however, is reported as greatly diminishing their viability. Marchoux, Salimbeni, and Simond (1903) state that "the degree of the nocturnal temperature" (i.e. the minimum daily temperature) "exercises the greatest influence" (as retarding) "on the functional activity of *Stegomyia*," and this has been confirmed by other observers. They were able to hatch out scarcely 5 per cent of eggs exposed for seventy days to a natural night temperature of from 10° to 20°C. (50° to 68°F.). They "believe consequently that all eggs submitted to chilling would probably perish at the end of several months."

The only observation the writer can find on the minimum death point for eggs on short exposure is unsatisfactory. Peryassú (1908, p.



397) states that eggs exposed in a mixture of salt and ice for one-half hour do not develop. He does not report, however, either the temperature of the mixture or the number of observations on which this conclusion is based. One would much like to know, because it might give us a clue to the northern limit of the viability of this species during the winter.

From the finding of Marchoux, Salimbeni, and Simond, quoted above, it would seem improbable that the species would survive the ordinary winter of Norfolk, Virginia, for example, or even farther south. In point of fact, they do survive it, and were found in the small towns and villages near by, where there were suitable breeding places, into which their introduction yearly from vessels had been impossible. It may be that their chances of survival are greatly increased by house breeding, or that the above finding is not of universal, maybe not of general, applicability.

#### THE THIRD FACTOR: THE MAN SUSCEPTIBLE TO YELLOW FEVER

In the formula which has been given for the mechanism of the conveyance of yellow fever, the presence of men susceptible to that disease and accessible to infective mosquitoes is included as a necessary condition for the continuation of this infection. This is definitely stated as a postulate. That this is so depends on the premises: (1) that the causative organism has no host other than man which is infective to the insect vector and is of sanitary importance; (2) that there is no free-living stage of this organism infective to man or to the mosquito; (3) that man is infective to the insect but a short time—that is, he does not become a carrier or reservoir host; (4) that he acquires immunity to the infection by the attack.

All of these propositions are believed to be true. For all there is good evidence—of necessity negative—for some, amounting to proof; and, except possibly the third, all are very generally accepted.<sup>20</sup>

Accepting them then: when, in any community, men susceptible to yellow fever are not accessible to yellow fever mosquitoes, the life-cycle of the causative organism is broken; and when the parasite in the mosquitoes already infected dies—probably with or soon after the death of the insect—the infection in this community ceases. The presence, then,

<sup>20</sup> In my judgment the negation of human carriers infective to *aegypti* rests upon evidence which is very strong, although necessarily negative; and no single observation as evidence to the contrary has come to my notice (Carter, 1916).



of men susceptible to yellow fever and accessible to infective *aegypti* is an essential factor in the continuance of that disease.

*Mechanism of the spontaneous elimination of yellow fever.* Very obviously, continuance of yellow fever in a community will lessen the number of people susceptible to that disease—each one attacked becoming immune to it, at least temporarily—and when this continuance has reached such a degree that no *aegypti* infective with yellow fever has access to the man susceptible to yellow fever, the disease will cease. This the writer (Carter, 1917) has called “the spontaneous elimination of yellow fever by failure of the human host,” and it has been of quite common occurrence, even where, from temperature conditions and the customs of the people, *aegypti* continue active during the whole year.

Since not only the presence of men susceptible to yellow fever is requisite for the continuance of that disease, but these men must also be accessible to infective mosquitoes, this elimination of yellow fever may take place even with a considerable number of susceptible men remaining, none of them being bitten by infective mosquitoes. Such mosquitoes in the neighborhood of these men may be few, on account of a lack of many sick men to infect them or from a general reduction in the number of *aegypti* in the neighborhood of these men. Naturally, too, the number of people immune to these mosquitoes lessens the chance of the susceptible ones being bitten. Under the doctrine of chances, then, it is easy to see that a considerable number of susceptible men might be in such a community and yet none of them be bitten by an infective mosquito.

“*Critical number*” of *aegypti*. Obviously, it is not necessary to exterminate *aegypti* in order to eliminate yellow fever from an infected place. If the number of mosquitoes be brought below the “critical number” for yellow fever—an idea we owe to Ross as applied to malaria, and extended to yellow fever by Gorgas (1908)—at that place, the disease will die out. Note, too, that this “critical number” for any place will vary directly as the proportion of men immune to yellow fever in the total population: thus, if with one hundred cases of yellow fever introduced into a community in which all were susceptible to yellow fever, the number of *aegypti* were such that exactly one hundred men would be infected from these cases, the disease would neither die out nor increase. This would be the “critical number” of *aegypti* for that place and time. With less mosquitoes than this, less than one hundred men would be infected, and the fever would die out. With more mosquitoes, more than one hundred men would be infected, and

it would increase. Now if one-fourth of the inhabitants were immune to yellow fever, obviously the same number of mosquitoes which infected one hundred men before would now infect only seventy-five—one-fourth of their bites going to immunes, and hence being wasted—and at this rate the disease would die out. The number of mosquitoes required to infect the one hundred men, and hence just perpetuate the fever, would have to be increased by one-third above the first number.

Obviously then, this critical number, below which the *aegypti* must be brought to eliminate fever, is less in a community as the proportion of susceptible people increases. Obviously, also the number of susceptible men in a community compatible with the elimination of yellow fever by “the failure of the human host” varies inversely, more or less, with the number of *aegypti* in flight.

Obviously again, the expression “community” must be understood in a restricted sense, as denoting the people who are in sanitary communication with each other. As Connor and Monroe (1923) have noted, the critical number of *aegypti* for the residence district for natives of an endemic center may be very different from that of hotels and lodging houses. In the first, the great bulk of the people are immune to yellow fever; in the second, susceptible strangers congregate. One should not, then, in general, predicate a critical index for a town as a whole. In the above sense a town is, or may be, a number of communities, each one having a different critical index.

*Proportion of immunizations to supply of susceptibles.* Naturally, the occurrence of this elimination will depend on the proportion of the number of people immunized to the number of susceptible people introduced in the same time. The former depends on the number of cases of yellow fever; the second is the sum of the babies born in the community, the susceptible immigration thereto, and the number—if any—of immune people losing their immunity to yellow fever.

The possibility of the elimination of yellow fever by the above mechanism does not depend on the permanence of the immunity acquired by an attack, so long as it is absolute and of a considerable duration. The men who lost their immunity would simply be equivalent to the same number of susceptible immigrants. The chance of elimination taking place and the time necessary for it to occur, do, however, depend on the *duration* of immunity—the chance being greatest and the time least when immunity is permanent. The fact that spontaneous elimination is so common is a strong argument against the frequency of recur-



rent attacks.<sup>21</sup> Obviously, in large towns, both from the number of babies born and the greater immigration, elimination would be less apt to occur or would take longer than in small or moderate sized ones and, in the latter, the spontaneous elimination of yellow fever as a fact, without regard to its mechanism, has been a common occurrence, even in the tropics and with *aegypti* abundant. It has disappeared from Cartagena, Barranquilla, Maracaibo, Cúcuta (towns of from 20,000 to 75,000 people); from all of Hayti and Santo Domingo; from Porto Rico, St. Thomas, and a host of smaller places; and this without any sanitary work and with *aegypti* abundant. It did not disappear spontaneously from Havana, Rio de Janeiro, Guayaquil, or Panama. The first three are large towns and all with large susceptible (European) immigration.<sup>22</sup>

For reasons which it would take too long to develop, while a sufficient number either of babies or of susceptible immigrants should be able to continue yellow fever, yet the proportional effect of the same number of each in so doing is different—decidedly more babies being required than adult immigrants (see Carter, 1917). The size of the community, then, is less important than the amount of susceptible adult immigration. Both factors are, in practice, apt to increase together, as large towns generally receive much immigration.

*Conditions of regional endemicity.* Besides large towns, and towns receiving susceptible immigration, there are certain communities of small places between which the travel relations are so close that they must be held, for this mechanism, as a single large community. Indeed, in such a group of interrelated places, the propagation of yellow fever would be decidedly slower than in the single larger town, and hence elimination of yellow fever therefrom might take longer and be less apt to occur than in a single town equal to them in population. Such a section is Yucatan and Campeche, the large sisal *haciendas* being, indeed, towns;

<sup>21</sup> The presence of lower animals susceptible to yellow fever, if immunity is produced in them by one attack, is simply equivalent to adding approximately the same number of susceptible men to our population. If, however, such animals were *permanent reservoir* hosts—as some are of trypanosomes—and infective to mosquitoes, the infection would persist.

<sup>22</sup> This principle, with the facts on which it is based, was first presented in lectures to a class of student officers at the Hygienic Laboratory of the U. S. Public Health Service in March, 1914 (Carter, 1914), and was elaborated in subsequent papers (Carter, 1917 and 1920). It was accepted by the Yellow Fever Commission of the International Health Board at Guayaquil, 1916, as determining the basis of measures proposed for the elimination of yellow fever from the west coast of South America, and is now generally accepted as a recognized principle in the epidemiology and control of yellow fever.



and this section did not free itself from yellow fever, but was a permanent endemic focus of the disease, apparently needing sanitation for its elimination.<sup>23</sup> Control of *aegypti* breeding was accordingly undertaken and is still going on at Merida, and this region is believed now (May, 1922) to be free from yellow fever.<sup>24</sup>

Similarly there have been—one is glad indeed to use the past tense—large regions forming what may be called permanent endemic regional foci. Such was that of the south shore of the Caribbean and the Lesser Antilles. Possibly either one, the south shore of the Caribbean or the islands, in the times of sailing ships and naval activity in the Caribbean, had sufficient intercommunication and ingress of susceptible people to propagate the infection in permanence. Possibly later—since about 1880, say—reinfections from Vera Cruz, Havana, and Panama, all of which were then infected, and through all of which passed much susceptible travel, may have been necessary to continue it. At any rate, yellow fever existed in this region from early times up to and including 1917. We called this a “permanent regional focus,” because yellow fever existed at some place in it at all times; and from such infected places other places in the region were infected from time to time.

<sup>23</sup> The idea that a number of small communities, each one too small by itself to remain permanently infected, in such travel relation with each other that the whole forms a permanent endemic focus of yellow fever—a “regional focus,” it was called—was, he thinks, first formulated by the writer in 1917 (Carter, 1917), using Yucatan with its sisal *haciendas* as an illustration. Guiteras noted the same condition in the sugar district of Coro, Venezuela, in 1917 (unpublished report on the Yellow Fever Situation at Coro, Venezuela, submitted to the Yellow Fever Commission, Rockefeller Foundation, October, 1917. See also Guiteras, 1921, for published reference) as did Connor (1923) in the sugar district of the Papaloapan River, calling it—I think quoting Guiteras—“*hacienda endemicity*.” It is but a special instance of the “permanent regional focus” with *haciendas* as the small community units.

<sup>24</sup> Whether the collapse of the sisal industry, following as it did upon the wholesale immunization of the inhabitants by the great increase of fever, due to the advent of susceptible troops during the political disturbances, would have been by itself sufficient for the elimination of yellow fever from this region, may be a question. Connor, in charge of the sanitary work there, thinks it would have been, indeed that it possibly had eliminated the disease before *aegypti* control was undertaken.

Still, when yellow fever is at a low ebb from lack of susceptible people, as naturally follows a general epidemic, is the time of all others when work against *aegypti* should be pressed. A very small reduction of mosquitoes then may be sufficient to eliminate infection. For it to be lessened does no permanent good; eliminated, however, it is gone forever, unless reintroduced.

Before one place had freed itself by failure of the human host it would infect another which had a sufficient number of susceptible men for fever to propagate for some time, to be itself reinfected when time enough had elapsed for it to accumulate susceptible men. Yellow fever was thus always present at some place in this region.

Evidently, in such a regional focus, as well as in one formed by a series of smaller places, the nature and amount of the intertravel are prime factors in the permanence or disappearance of yellow fever. There is, speaking generally, this contrast between a permanent regional focus and a permanently infected town. In the latter the parasites, in infected *aegypti*, are always present, and the infection is kept up by the advent of susceptible men; in the former, susceptible men and uninfected *aegypti* are always present in some part, and the infection is kept up (counting the regional focus as a whole) by introducing the parasite into such places by the advent of infected men from other parts of the region. In the one, the parasites (in infected *aegypti*) are in permanence; in the other, they are carried (in infected men) from one part of the region to another. If, however, we consider each house or little district in a city as a community unit, there is really little difference in the essentials of the above.<sup>25</sup>

The west coast of Africa formed a similar regional focus, apparently permanent, but the continuance of this in recent times was doubtless assisted by occasional reinfections from America, principally from Havana, the French Antilles, and the Brazilian ports. Some *aegypti* control work has been done in West Africa. It is reported as small in amount, and although it has been done at strategic points, as Sierra Leone, Lagos, and others, it does not seem to have been enough seriously to affect the existence of the African focus. Yet during and since the war there seems to have been a marked diminution of yellow fever in Africa, and Guiteras (1921-b) hopes that this great regional focus has also disappeared. His observation was in October, 1920; and to date (July, 1922) the writer finds no report of definite occurrence of yellow

<sup>25</sup> No work sufficient seriously to affect the general prevalence of yellow fever has been done in the region referred to—the south shore of the Caribbean and Lesser Antilles—and yet, with the removal of the chance of reinfection from Vera Cruz, Havana, and Panama, the diminution of immigration and of intertravel due to the Great War, and some sanitary work at Coro, in 1917 and 1918, possibly giving it the *coup de grace*, yellow fever seems to have disappeared from this great regional focus. The last report of it was around Coro, Venezuela, in 1917, by Guiteras (1921a: also in unpublished report to Yellow Fever Commission, Rockefeller Foundation, October, 1917).



fever in Africa since that time. There were cases, however, on the Gold Coast in 1919, and quite a number in 1917 and 1918. Hence, much as the writer would like to agree, he is afraid to accept the tentative opinion of Guiteras until a decidedly longer time has elapsed and there has been a complete restoration of the European communications which were seriously interrupted by the war. It is much more difficult to determine the existence of yellow fever, or to make sure that it does not exist, in Africa than in America;<sup>26</sup> and if the disease shows again within a few years, we may not be able to say whether it is a recrudescence of the old infection or a reintroduction from America—as from Brazil.

<sup>26</sup> Guiteras (1921) has suggested that the great mortality of malarial fever reported in West Africa in excess of that in America and India, may have been due to the inclusion, under that diagnosis, of cases of yellow fever. Some British observers, on the contrary, have been inclined to think that a number of the cases reported as yellow fever in West Africa have been severe malaria, and to doubt that there has been any yellow fever on this coast for a number of years. Several other British writers have also expressed grave doubts as to the recent existence of yellow fever on this coast. The writer cannot think that this opinion rests on a sufficient basis—i.e. the non-report of yellow fever for a few years in West Africa. He will be very glad to acknowledge his error if mistaken.



## CHAPTER II

### DISPERSION AND GEOGRAPHICAL DISTRIBUTION OF *AËDES AEGYPTI*

I have spoken of the conditions—temperature and the nature and location of breeding places—necessary for a supply, continuous or intermittent, of active *aegypti*, but naturally the first condition is the presence of the mosquito itself to begin a generation.

### MECHANISM OF DISPERSION OF *AËDES AEGYPTI*

Whatever the origin of *Aëdes aegypti*, its habits are such that it would be widely distributed by human agency, and in point of fact, it is now of very wide distribution. Some of this extension we can trace in historic times; much of it we cannot. It is obvious from what has been said of its breeding habits, that the conditions obtaining on sailing vessels in hot climates were well fitted for its distribution. Drinking-water in artificial containers, accessible to the mosquito and in proximity to man, make such a vessel well-nigh an ideal breeding place for *aegypti* and, seeded at some port in which the species existed, the vessel, thus acting as a host, would carry them to other ports. At such ports, if *aegypti* went ashore and if the conditions there were suitable for propagation—artificial containers and proper temperature—they might form a colony, possibly a permanent colony, from which further distribution could take place.

At a port where there were no places suitable for breeding, no colony could be established—only the insects which left the vessel would be present, and they only until they died out. The same would be true if the temperature was too low for oviposition when the insects came ashore: there would be no colony established; those that came ashore would die, and in this case, we think, quickly.

If breeding places were available and the temperature on landing high enough for the full functional activity of the insects, a colony could be established and would be permanent or not, according to the winter temperature of the locality, and the colony would last until the species (eggs) were destroyed by the cold, which might be the first winter; or, in border-line places, the species might live several years, to be destroyed eventually by some winter of unusual length or severity. Such seems to have been the case in Philadelphia and possibly New York in the latter

part of the eighteenth century. In many places conditions have been only too favorable to the propagation of *aegypti*, and there is no question but that distribution by man has been a determining factor in their dissemination. Wherever they may have originated, they are now found in every seaport and large river port in the world in which the conditions are suitable for their propagation.

Steamboats plying on large rivers are only less efficient hosts for breeding and distribution than the sailing vessels of which we have spoken, and they have carried *aegypti* inland—certainly up the Mississippi and its branches—from the ports nearest the sea. We have strong evidence, indeed proof, that there were no considerable number of *aegypti*, if any, in Memphis as late as 1853, or in the towns on Red River in 1847 or previously.

Transferring of *aegypti* by railroad coaches has not been directly observed, but the evidence that it has occurred in both the United States and Mexico is convincing. This would be, of course, merely a mechanical transfer of individual insects. They would very rarely, if ever, breed aboard the coaches as on vessels.

The habit which the insect has, especially in slightly cool weather, of resting on one's clothing (the writer has seen them hiding under the lapel of his coat) is doubtless also an occasional, though rare, method of dissemination. At any rate, *aegypti* were of quite general distribution in 1898 at farmhouses in northern Mississippi, well off from the railroad, while in previous epidemics (1867 and 1868) such places had been clearly non-infectible with yellow fever—that is, had no considerable infestation. This, to the writer, seems a possible, and a reasonably probable, explanation for the infection of Dr. Chaillon, "the physician of Montoir," as reported by Mélier (1863) in his account of the celebrated epidemic of St. Nazaire, 1861.

A vessel, the *Anne-Marie*, from Havana for St. Nazaire, developed some cases of yellow fever aboard, in Havana and en route, but arrived at her destination and docked with no one sick aboard. The crew was immediately discharged, and the cargo (of sugar) was unloaded by stevedores. Within a few days cases of yellow fever began to develop, in the officer who supervised the unloading, in stevedores and other workmen aboard, in the personnel of other vessels lying close by, and some cases in persons ashore in the immediate vicinity of the *Anne-Marie*. In all, at least 42 cases and 25 deaths occurred. In one ship, the *Arequipa*, which had put to sea, secondary cases occurred. With this exception, all the cases may be accounted for as contracted from the



bites of infected *aegypti* on the *Anne-Marie* or nearby, save in the case of Dr. Chaillon, "the physician of Montoir," a village some seven kilometers distant from St. Nazaire.

From August 5 to August 11 Dr. Chaillon attended, at their homes in the vicinity of Montoir (some miles from St. Nazaire), four stevedores who had been engaged in the unloading of the *Anne-Marie* at St. Nazaire and had there been infected. On the 13th of August, seven days after his first, and two days after his last, visit to any of these patients, Dr. Chaillon himself developed yellow fever, of which he died. Mélier, the reporter of this epidemic, states—as does Madame Chaillon in a letter which he quotes—that Dr. Chaillon had not been to St. Nazaire since the arrival of the *Anne-Marie* and that no one in Montoir or its environment had been sick of that disease, before or afterwards, except these stevedores and Dr. Chaillon. No other member of the stevedores' families developed it, nor did any of Dr. Chaillon's household. Absence of *aegypti* in this section is reported in the summer of 1918 (personal communications to the writer).

The data, which are definite and seem to have been most carefully collected, are believed to be reliable; and there is, I think, no question of the nature of the disease in either the stevedores or the physician. Dr. Chaillon could have acquired his sickness from the bite of a mosquito infected aboard the *Anne-Marie*, transported from the vessel by one of the stevedores, clinging to his clothing; and, accepting Mélier's data, the writer can conceive of no other way. The transportation of an insect already infected is predicated because the development of the disease in Dr. Chaillon was too soon after that of his patients for it to have been caused by an insect infected by any of the latter. This case was, indeed, contracted at Montoir, but we feel quite sure that the mosquito infecting Dr. Chaillon received its infection aboard ship en route to St. Nazaire.

It is fair to say, then, that for the United States, at any rate, *aegypti* have been carried almost everywhere where there was a reasonable chance of their forming a permanent colony, and to a great many places in which no such colony could be formed. For countries in which internal communication is less good than in the United States, this may not be so; but for the United States as a whole, and for the seaports and large river ports everywhere, it is fair to say that *aegypti* have been introduced into all of them, and if they do not exist there now it is because local conditions are unfavorable to their existence.

The introduction of *aegypti* into a community, even when breeding



conditions are favorable, does not lead to a rapid distribution of them therein. They, indeed, increase in number, and may increase rapidly, but the area of infestation is not extended rapidly. We have, naturally, only indirect evidence for this, but this indirect evidence is convincing.

In 1876 there were epidemics of yellow fever at Savannah and Brunswick, Georgia—at Savannah a severe one. Refugees from these places went to Fernandina, Florida, then a small place. A number of them developed yellow fever there soon after their arrival, but no spread of the disease occurred there, and no one living continuously in Fernandina contracted it. The next year, 1877, a Spanish vessel came into Fernandina from Havana, with her captain sick of yellow fever. Some of those going aboard the vessel and working on the wharf contracted yellow fever and developed it at their homes in the little town. At some of these houses foci of yellow fever were established, from which the fever spread, especially in the vicinity of the landing wharf off which the vessel lay, but not confined to it; and before the end of the warm weather there were enough of these foci of infection to make a limited epidemic scattered about the town. Evidently the town was not, in 1876, infested with *aegypti* sufficiently to establish foci of infection in the places where yellow fever occurred, while in 1877 it was. The vessel which brought in the sick man, and which was accused of bringing in the “bad air from Havana, without which yellow fever could not spread,” most probably had *aegypti* aboard, and some may have gone ashore and bred there; but from the history of the spread of the fever, there would seem to have been some places in Fernandina already infested with them in 1877—probably seeded the previous fall from Savannah or Brunswick.

#### REQUIREMENTS FOR PERMANENT EXISTENCE OF *AËDES AEGYPTI*

*Breeding places.* Of the two requirements necessary for the permanent existence of the species in any place, the first—suitable breeding places—is largely, or it may be wholly, dependent upon the domestic storage of water. This is a question of the customs of the people and has to be determined for each place by itself. Since the tendency of advancing civilization is to dispense with domestic storage of water, replacing it with a piped supply, this factor in advanced civilization has acted to free a certain number of places from *aegypti* infestation and, even when this was not done completely, to lessen the degree of this infestation, frequently to the point where it was of no sanitary importance. Naturally, the influence of this change is greater in border-line

places, in which the temperature is close to the limit at which the permanent existence of the species is possible, so that a slight added handicap would be sufficient to turn the scale against them. As an illustration of the first result (elimination), we find now no *aegypti* in Philadelphia or Baltimore, where they undoubtedly existed during some summers before piped water-supply systems were installed, hibernating over some winters in the former city (see p. 42) and doubtless in the latter. An example of reduction, not elimination, is found at Norfolk, Virginia, where, at present, *aegypti* are very few, although abundant in neighboring villages. The progress of civic improvement tends, then, to remove or lessen *aegypti* infestation in towns, and, by that factor, to render them non-infectible; but it is piped water doing away with its domestic storage, not sewerage, drainage, or general cleanliness, which has this effect.

*Temperature.* The other requirement for the existence of the species—suitable limits of temperature—is naturally dependent on the location of the region. It is usually expressed as a function of the latitude: as “between 38° north latitude and 35° south latitude.” Altitude, however, affects the temperature, especially night temperature, not less than latitude, and to take in this variant—it is by no means the only one—makes an extremely complicated expression.<sup>1</sup>

#### TEMPERATURE LIMITS OF PERMANENT LIFE-ZONES OF *AËDES AEGYPTI*

*Biological zones.* Merriam (1894, p. 213) has given us a formula for the temperature limits of the life-zones of both plants and animals, beautiful in its simplicity and comprehensiveness, and very generally applicable: “In northward distribution” (in the Northern Hemisphere) “terrestrial animals and plants are restricted by the sum of the positive temperatures for the entire season of growth and reproduction.”

It having been determined that the development of the land forms of both plants and animals begins at 6°C., only temperature in excess of 6°C. is effective; and the expression “positive temperature” is used for this excess. The total heat received, then, is measured by the sum of the excesses of each mean daily temperature above 6°C. during the year.

<sup>1</sup> The effect of altitude in limiting the existence of *aegypti* is quite certainly due to its effect on temperature. The suggestion, attributed to Finlay (1912, p. 30), (although he never made the categorical assertion) that the insects could not feed or fly at an altitude above 4,000 to 6,000 feet, is disproved by Fernando Lopez (1905), who found they could do both in the City of Mexico at 7,415 feet, an altitude at which, even on the equator, the temperature is below that really suitable for the activity of this insect.



The northern limits of Merriam's life-zones would be determined by isotherms showing an annual sum of such normal "positive temperatures" equaling a definite number of degrees—as, for instance, 10,000°C. This number, then, would be the "physiological constant" of temperature of the organism whose life-zone was bounded by the isotherm in question. Evidently this formula, being dependent on temperature alone, allows for the effect of latitude, altitude, ocean currents, winds, and all other factors that affect temperature.

Howard (1903 and 1905) fixed the limits of life of *aegypti* in the Northern Hemisphere as the Tropical and Lower Austral Zone as defined by Merriam (see map, p. 30). As he found later, however, that this insect was absent from some regions, as for instance California, which are included in this zone, this statement is not repeated in his great monograph, *The Mosquitoes of North and Central America and the West Indies* (Howard, Dyar, and Knab, 1912–1917). Instead we find (p. 292): "its permanent distribution is determined by the minimum temperatures and its temporary distribution by the maximum temperatures of any given region wherever it is sufficiently populated."

We believe this latter view of Howard's to be absolutely correct and, in spite of the beautiful simplicity of Merriam's formula and its general adaptability to species able to survive (it may be in an inactive stage) very low temperatures, the writer's knowledge of the existence of this insect east of the Rocky Mountains compels him to believe that this formula is not applicable in principle to *Aedes aegypti*—that its continuous life as a species at any place in that region is dependent on whether the species, hibernating as the egg, is able to survive the *low* temperature of the winter, and not on whether there be sufficient heat for development and reproduction during the summer, which is the basis of Merriam's formula. This does not take into consideration the effect of warmed houses in preserving eggs which would otherwise perish; yet this we would have to do, as in practice it habitually occurs. Merriam's formula, as we have said, does not take into consideration the effect of cold in destroying the species at any place, only the absence of the heat necessary for its full evolution; and yet we know that *aegypti* do find heat enough for development and reproduction when introduced by vessels during the summer into places far to the north of their permanent life-zone. Nevertheless, on the Atlantic coast and in the central valley of the United States, the northern limit of the Lower Austral Zone is not far from that of the permanent life-zone of *aegypti* as fixed by detailed observation, although, we believe, determined by different factors. Hence the publication of the map on page 30.





Corrected to December, 1897.

FIG. 1. LIFE ZONES IN THE UNITED STATES (MERRIAM)

From Public Health Repts., U. S. Public Health and Marine-Hospital Service, Suppl. vol. 18, Nov. 13, 1903



*The two limiting conditions of temperature.* From what has been said of the biology of this insect, there are evidently two conditions of temperature, both of which must obtain in any region for the continued existence of *aegypti* in it:

1. The low temperature of winter must not be so low or so prolonged as to destroy the species—the hibernating eggs.
2. The high temperature of summer must be sufficient for the development and reproduction of the insect.

The natural life-zone of the species, the region in which it is permanent, so far as temperature is concerned, includes all regions in which both of these conditions are fulfilled, and none other.

#### GEOGRAPHIC DISTRIBUTION OF *AËDES AEGYPTI* IN AMERICA

East of the Rockies—and the same is true east of the southern part of the Andean Cordilleras—the first condition is the ruling variant. The limits of the life-zone of *aegypti* are determined by that combination of latitudes and altitudes which gives temperatures allowing the hibernation of the species in the cold season. We think that in no place in these regions where this condition is fulfilled, will the temperature in the hot season fail to be sufficient for the development and reproduction of the insect. On the west coast, however, in both the northern and southern part of it, the second condition will determine the limits of the life-zone of this insect. The winter temperature would not prevent hibernation at any distance north or south of the equator on the coastal plain of the Pacific at which the summer temperature would be sufficiently high for development and reproduction. Ocean currents are the main factors controlling temperature in this region.

In the American tropics where, except in one region, the limits of this life-zone are imposed absolutely by altitude, it is the second factor, sufficient summer heat, which is determinative. Seasonal differences of temperature in the tropics are not great, and long before an altitude was reached at which the eggs would perish, heat sufficient for the development and reproduction of the imago would be lacking. High altitudes, too, are especially cold at night, and we have seen that the limit to the functional activity of *aegypti* is set by the minimal diurnal (i. e., nocturnal) temperature rather than the mean diurnal temperature.

The region excepted above is the Pacific coast of South America, from Tumbez south. Here the controlling factor of the temperatures is the Antarctic Current (Humboldt's Current) in conjunction, of course, with the latitude. Here too, there being little seasonal change of tempera-

ture until one goes very far south, the second factor again is the determinative one in the limitation of *aegypti*.

This condition has its analogy in the life-zones of plants. For instance, the northern limits of coffee, which requires no great heat to mature its seed, but bears very little cold, are determined by the winter temperature. On the other hand, Indian corn, an annual, the seed of which, when mature, is in no wise affected by cold in the winter, can be grown to its maturity only where there is, in summer, a considerable amount of heat, either in degree or duration. *Aegypti* is like both; the species—although a hibernating one—is destroyed by a low winter temperature, and cannot develop and reproduce unless with a sufficient summer temperature when it is active. In the latter state it is especially sensitive to low night temperature.

At present the neighborhood of Norfolk, Virginia, is about the northern limit of *aegypti* survival, at sea level, on the Atlantic coast. That they did survive several winters in Philadelphia about the end of the eighteenth century, and probably one winter at least (that of 1797–1798) in New York, has been stated. Cairo, Illinois (also low altitude), or a little north of it, is about their northern limit in the Mississippi Valley. As we go south from these places, the limits of the zone are naturally determined by altitude. This is low (sea-level) at its northern limit and rises as we go south. The foothills of the Appalachians in Georgia and South Carolina are outside of this zone, as are the Ozarks in Arkansas. Finally, in Mexico, *aegypti* have been found at Orizaba (alt. 3684 feet), apparently now in permanence. They have not been found in Jalapa (alt. 4300 feet); and neither San José (about 4250 feet) nor Cartago (4930 feet), Costa Rica, has proved infectible. The slight but definite spread of yellow fever which has occurred at Alajuela and at Heredia, Costa Rica (alt. about 3000 feet), would show these places to be about on the border line of temperature for the zone. The statement, ascribed to Humboldt, of yellow fever's being contracted at Quito, is obviously erroneous. I cannot find that he made it.

On the west coast of South America *aegypti* were found in the coastal region of Peru as far south as the Chilean border (Dunn, 1923) and must have existed further south, for yellow fever has existed (in 1912) in epidemic form as far south as Tocopilla, Chile, 22° south latitude (U.S. Public Health Service, 1912, p. 195). From its extent, the infestation of this place with *aegypti* must have been general and, since the winters here are not severe, permanent. They were not found, however, anywhere in the foothills of the Cordilleras of northern Peru in 1920 or 1921, due,



the writer thought, more to absence of suitable breeding places than to temperature.

On the east coast, the severe epidemics in Buenos Aires in 1870 and 1878 and in Montevideo, 1871 and 1878, would show that both towns were well infested with *aegypti* at that time; not necessarily, however, that they were within the zone of permanent breeding. Indeed, the late appearance and rarity of such epidemics at these places, and even at Rio Grande do Sul, strongly implies that there was no very early general, permanent infestation with *aegypti* in this region, as does the fact that the disease was not propagated in the villages near Montevideo when it was introduced. Judging likewise by epidemiological, not by direct entomological, data, Santos (about 24° south latitude) is about the southern limit of the area which has been generally permanently infested with *aegypti* on the east coast of America after about the middle of the nineteenth century.

## CHAPTER III

### DISPERSION AND GEOGRAPHICAL DISTRIBUTION OF YELLOW FEVER

Of the three factors necessary (pp. 6–8) for the propagation of yellow fever, one, men susceptible to yellow fever, is present and abundant in every community into which the disease is newly introduced. The dispersion and distribution of yellow fever are dependent, then, on the coincident dispersion or distribution of the causative organism and of active *aegypti*.

The mechanism for the dispersion of the parasite, that is, of infected men and infected mosquitoes, outside of which the parasite has no sustained natural existence, is the same as that for the dispersion of *aegypti*—i.e., by vessels, railroad coaches, and other means of human transport. This we have discussed, but the details are not all the same and differ among themselves according as the parasite is transported (1) in infected mosquitoes; (2) in infected men; or (3) in a combination of the two.

#### TRANSPORTATION OF INFECTED MOSQUITOES

This is in all respects like the dispersion of *aegypti* as previously given, except that it is concerned with the spread of *infected aegypti* only, and that, while the infected insect may breed aboard vessels, as we have stated, her progeny as such are not infected.

Infective *aegypti* may infect the men aboard a vessel or coach; or, the men being immune to yellow fever, or the insects not yet in the infective stage, or there being from any cause no contact en route between susceptible men and infective insects, the latter may be transported to the place of destination without the occurrence of human infection en route. At these places the result, immediate and permanent, will be determined by the conditions of temperature, breeding places, and susceptible population there existing, as detailed before.

If the infected *aegypti* remain aboard ship, they may infect men aboard or coming aboard, and independently of temperature conditions outside, as has happened at Halifax, 1842 and 1861, Portland, Maine, 1801, etc. If the outside temperature at the port of arrival is high enough for feeding, the infective insects may leave the vessel and infect men within their range of flight, or in places to which they may be carried, as long as they



remain active, as happened at St. Nazaire, 1861, Swansea, 1865, etc. If the conditions are suitable for breeding, they may establish a colony, temporary or permanent, according to temperature conditions already discussed. If their progeny should be infected from the men infected by the first (imported) generation of *aegypti*, secondary cases may occur, and there may even be an epidemic at once, as in 1861 in the ship *Arequipa*, sailing south with infected mosquitoes taken aboard from the *Anne-Marie* at St. Nazaire (Méliér, 1863). Should the place be already infested with active *aegypti* (infectible territory: see page 40) the chance of the propagation of yellow fever therein is enormously increased and an endemic focus may or may not be formed, depending upon the range of temperature and other conditions at the place.

Experience has shown it to be the rule, however, for infected *aegypti* to disappear from vessels during a voyage of moderate length in cold climates. With steamships, indeed, this is the rule even in warm climates. None breed aboard steamships; those taken aboard at the port of departure die or are blown outboard—the latter usually—if the voyage is a fairly long one. Aboard sailing vessels the contrary is the case. Many a sailing vessel used to show fever on the way up from Santos or Rio and then infect men going aboard her at a Gulf port of the United States, forty to sixty-five days out (see page 37). Sometimes, however, infected *aegypti* disappeared from sailing vessels even in the tropics. For example, according to notes taken at Ship Island Quarantine, in 1890 the “British ship *Prince Edward*, Rio for Ship Island, Mississippi, had yellow fever developing aboard for over twenty-one days from Rio, losing four men. Put into Barbados on the thirty-sixth day. In quarantine there without disinfection. Shipped eight men to replace those lost in Rio and en route. Arrived Ship Island Quarantine twenty-four days later. No more sickness en route, on arrival, or in Quarantine.” The men shipped in Barbados were all apprentices, bright-faced English boys, just out from home, and, unquestionably, susceptible to yellow fever. Evidently the vessel had had infected mosquitoes aboard, as cases developed too long after leaving Rio to have been contracted there; but, as evidently, she had cleared herself of them before she shipped these boys.

These cases, however, are not the rule. Usually *aegypti* breed aboard a sailing vessel as long as the temperature permits, the newly developed mosquitoes are infected by the first lot of cases, and the duration of the infection of the vessel is limited only by the supply of susceptible men. In warm climates, then, the vessel may be, and frequently is, infected,

that is, has infected *aegypti* aboard, weeks after yellow fever in men disappears—all being immunized or dead.

#### TRANSPORTATION OF INFECTED MEN

If there are uninfected *aegypti* aboard the carrier and the men are infective to mosquitoes, these may be infected by the men. Such mosquitoes, after their period of extrinsic incubation is passed and they have become infective, may infect other men aboard the carrier or elsewhere. The end result depends upon the conditions of *aegypti* activity and the accessibility to infective *aegypti* of susceptible men when and where these secondary cases occur. If this be ashore, in port, we may limit our consideration to the question of *aegypti* activity, as our port of destination is supposed to be free from yellow fever, hence with an abundance of susceptible men.

If there be no *aegypti* aboard, or if the infected men aboard the carrier are only in the incubative state<sup>1</sup> while en route, there will be no infection of mosquitoes aboard ship.

In the first case the end result will depend (a) on whether the sick men are infective to mosquitoes when they leave the carrier and (b) on the nature of the *aegypti* activity at the place to which they are transported. In the second, the infected men being still in the incubative stage when leaving the carrier, the end result will again depend on the *aegypti* activity at the place in which they develop yellow fever, on whether the place be infectible or non-infectible territory (see page 40), and whether or not conditions of temperature allow of the formation of an endemic focus.

#### COMBINED METHOD OF DISPERSION

Ordinarily the dispersion of yellow fever on land has been by the movement of infected men. The infected mosquito, although a factor, is of very much less importance. By sea, however, the method usually has been a combination of the two. Infected mosquitoes came aboard and infected the personnel, or infected men came aboard and infected the *aegypti* already there. In the first case the uninfected *aegypti* aboard quickly became themselves infected from the infected personnel; in the second, the susceptible men aboard became infected—as secondary cases—as soon as the period of extrinsic incubation of the thus infected *aegypti* had passed. With *aegypti* breeding aboard a vessel, the fever

<sup>1</sup> If past the stage infective to mosquitoes, the man is considered to be no longer “infected,” although he may be still sick with yellow fever.



terminated when the human material was exhausted, the vessel remaining infected for weeks afterward. With a vessel not breeding *aegypti* aboard, the fever usually terminated by the insects with which she started being blown outboard—rarely lasting long enough for them to die. When the vessel reached port, if she had freed herself from infection, of course she carried no parasites. If she was still infected—parasites in men or in mosquitoes or in both—the end result depended on the breeding conditions ashore and, if these were suitable, on whether the range of temperature allowed of the formation of an endemic focus or not. These have been sufficiently discussed.

#### TIME ELEMENT IN CONVEYANCE BY VESSEL

Provided a vessel be free from *aegypti* there is no danger of the introduction of yellow fever by infected men from a port over twelve days distant. This interval should cover the maximum period of incubation plus the period of infectivity to mosquitoes. To guard against infected *aegypti*, however, a much longer time is required if time be depended on. For example, in 1890 the "British bark, *Curlew*, arrived at Ship Island Quarantine, direct from Rio, 58 days out, no history of yellow fever in Rio or of sickness of any kind en route. Four days after opening the hatches one man sickened with yellow fever" (author's notes taken at Ship Island Quarantine Station). To say positively that no cases of yellow fever had occurred en route and been overlooked is, of course, impossible; but no absence from duty en route on account of sickness was logged, nor was any discoverable on questioning officers and crew.

Such cases must be extremely rare now, and were never common, yet the same thing happened that same summer to three other vessels at the Ship Island Quarantine: British bark *Avon* (Rio), 38 days to first case; British bark *Chippewa* (Rio), 68 days; Spanish bark *Castilla* (Havana), 12 days. Aboard the *Avon* two cases, men who cleaned out a store-room. Aboard the *Castilla*, two cases, captain and mate, all others aboard her being immune to yellow fever. Three deaths among the six<sup>2</sup>—all autopsied. The next summer Dr. G., Assistant Quarantine Officer at this station, died of yellow fever contracted in quarantine, although no vessel up to that time had showed yellow fever for over three weeks before arrival, and the one from which we believed it was contracted had been over thirty days without a reported case. Most, or all, of her crew were immune—many immunized on the way up from Rio.

<sup>2</sup> Statement implies two cases aboard the *Chippewa*.—Ed.

It is for this reason that it has been feared that on the change of trade-routes consequent on the opening of the Panama Canal, yellow fever might be carried across the Pacific to Asia by infected mosquitoes shut up in the holds of steamers (producing no sickness among their personnel to serve as a warning), while there was no apprehension of danger directly from infected men. Men would develop the fever and be past the infective stage before the vessel reached an Asian port; and, unless they infected *aegypti* aboard, would not be a source of danger, in spite of all Southern Asia's being "infectible territory" (Carter, 1924-b).

#### RATE OF SPREAD ON INTRODUCTION

If yellow fever (the causative organism) be introduced into a susceptible community in which active *aegypti* are abundant and generally distributed, it will spread, at first slowly, on account of the period of extrinsic incubation in the mosquito; hence, the long interval between infecting and secondary cases. Soon, however, cases of yellow fever contracted in the first focus will develop in other places and form other centers of infected *aegypti*, and thereafter, say after six to eight or ten weeks, the spread will be rapid. On the other hand, if yellow fever be introduced with the *first* introduction of *aegypti*, secondary foci outside of the area of *aegypti* infestation cannot be formed; this area can spread no faster than the *aegypti* do; and thus spread is slow.

An illustration of this slow spread is found in the epidemic in Boston in 1798 (Rand, 1800). The infection began at a dock; and cases contracted here developed in many parts of town, but in no case formed a focus from which other men were infected. The infected district, that is, the district in which yellow fever could be contracted, advanced from Stoddard's Wharf, where it began in June, some squares from the water, and to other wharves as well. "The contagious effluvia were conveyed"—down the wind—"into Battery-march, Liberty-square and Kilby-street." Quite certainly, in this case, *aegypti* were introduced the same year as the fever. The eggs would be little apt to have survived a Boston winter.

Obviously, the rate of spread, whether slow or comparatively rapid, may give us a means of determining the degree of infestation of a place with *aegypti*, and whether they were introduced with—or about the same time as—the fever, or long before.



## REINFECTION OF INFECTING PLACES

Communities in which yellow fever was introduced and established itself, themselves became centers from which it might and not infrequently did spread, directly or indirectly, reinfecting the places from which the infection had originally been received, when such places had freed themselves from infection by failure of the human host or had been freed by a seasonal fall of temperature. Africa quite surely infected the island of Grenada directly, and a number of the Lesser Antilles indirectly, with the celebrated "Fever of Bulam," although Africa is credited with having suffered infection from the Caribbean more than once. So Vera Cruz—quite certainly the source of the Havana fever of 1761—ascribes many of her epidemics to importations from Havana. Yellow fever seems to have been sent back and forth, like a shuttlecock, between Pernambuco and the Slave Coast in Africa. The recriminations between the different islands of the Lesser Antilles, and between them and the Guianas, regarded as so humorous by Moreau de Jonnés (1820) and Cornilliac (1886), were reasonable and just.

## CHAPTER IV

### THE RELATIONS OF TEMPERATURE AND SOCIOLOGICAL CONDITIONS TO THE OCCURRENCE AND PERSISTENCE OF YELLOW FEVER

Very obviously, in regions in which the temperature at all seasons is within the limits given for functional activity, given suitable breeding places—and in practice only the lower limit need be considered<sup>1</sup>—the *aegypti* imago will be active (reproducing and feeding) the whole year around. In such regions the second requirement for the continuous existence of yellow fever<sup>2</sup>—the presence of functionally active *aegypti* imagoes—will exist at all seasons of the year; and if the other two factors are also present, yellow fever may continue during the entire year. Such a fever is said to be endemic, and such regions are said to be in the endemic zone.

This second factor (active *aegypti*) will also be present at any season (the breeding places being unchanged) in proportion as temperature at that season approaches the optimum of *aegypti* development; and consequently—the other two factors remaining constant—yellow fever will increase and decrease as the temperature approaches and departs from this optimum. This was seen in Havana, Rio, Vera Cruz, and other places in which the fall of temperature was not sufficient entirely to prevent *aegypti* activity at any time, but did limit it during the colder season.

### INFECTIBLE AND NON-INFECTIBLE TERRITORY: DETERMINING CONDITIONS OF TEMPERATURE

The conditions of temperature affect the factor of *aegypti* activity in other ways, in none of which is it compatible with the endemicity of yel-

<sup>1</sup> Whether the species can survive the intense heat occurring, for example, in Mesopotamia and on the shores of the Red Sea, I do not know. For the first, Christophers and Shortt (1921) give the mean maximum temperature for July as 109.4°F., and for August 110°F. Possibly the insects may be able to find cool places about porous ("sweating") water jars. The hot season is very dry, and evaporation might keep them cool, but there would unquestionably be a heavy mortality among them. The temperatures quoted are well above death points found experimentally, even with abundant moisture, the lack of which lowers it.

<sup>2</sup> The three factors referred to (see pp. 6-8) are: (1) the causative organism of the disease; (2) active *Aedes aegypti* mosquitoes; and (3) susceptible men—the mosquitoes having access to both the other two.—Ed.



low fever, that is, continuance of the infection from year to year without reintroduction, and yet with any of which yellow fever may and does occur.

(1) *The seasonal fall of temperature may be sufficient to suspend the activity of the imagoes for some considerable time—generally by destroying them—but is not sufficient to destroy the eggs.*

In this case yellow fever will disappear absolutely if all imagoes are destroyed and none others develop for about twelve or fourteen days.<sup>3</sup> With the return of warmer weather the eggs will develop and *aegypti* reappear. At first they will be in numbers inversely proportional to the length and severity of the winter. This is true even in New Orleans and Mobile; the early *aegypti* do not appear in those places in swarms, as do *Aedes sollicitans*, for example. Apparently a large proportion of the fall-laid eggs fail of development. This is probably less apparent as one goes south. Later in the summer *aegypti* increase until they are in sufficient numbers for the propagation of yellow fever; but the parasite having disappeared in the meantime from the men who had been infected, the mosquitoes cannot be infected, and yellow fever will not appear in such places unless the parasite—in infected men or infected insects—be reintroduced.

Such places were Memphis, Tennessee; Jackson, Mississippi; etc. Every summer, during the activity of *aegypti*, these places were “infectible territory,” and if men infected with yellow fever were introduced, the disease might propagate, to die out entirely the next cold season. There are, however, “border-line places,” in which the temperature in some winters is low enough to eliminate yellow fever (by destruction of infected imagoes) and in others is not low enough. In the mild winters yellow fever infection will not die out in these places, and we have a “hold-over” epidemic when the number of *aegypti* sufficiently increases the next summer to make one. This probably has occurred sometimes at New Orleans, as from 1897 to 1898. The writer is quite sure it has not been usual. He knows no reason to believe that it has ever occurred at Mobile.

(2) *The temperature during the cold season may fall so low and remain low so long as to destroy the viability of the eggs of the aegypti.*

Under such conditions there is no hibernation of this species. It is

<sup>3</sup> There was a considerable fall of temperature in Memphis during the epidemic of 1897, soon after it began in the fall. This was followed by a marked drop in reported cases, but with the onset of warmer weather there was an increase of the fever, which kept on about as usual.

destroyed during the winter and, unless it is reintroduced after the weather becomes warm, such places remain uninfectible even if men infected with yellow fever be introduced. If *aegypti* be introduced, by vessels from southern countries usually, and the conditions for their breeding—temperature and artificial containers near residences—be then suitable, they may propagate to such an extent as to render the place infectible on the introduction of men with yellow fever later in the summer, the fever, of course, disappearing with cold weather, and the locality again becoming “non-infectible” until *aegypti* are again introduced and propagate.

It would rarely happen, however, during the short summer of a town in which the winter was cold enough to forbid hibernation of the species, that *aegypti* introduced that season would be able to spread generally over the town to a distance from the wharves, unless, indeed, they were introduced in a number of places, scattered over the town. It is on this account that it seems unreasonable to believe that the generalized epidemics of 1798 in New York and of 1793, 1797, 1798, and 1799 in Philadelphia could have been conveyed by the progeny of *aegypti* introduced during the years in question—even if they were introduced in a considerable number of places on the water-front. For such generalized epidemics (there were 2,086 deaths reported in New York in 1798; 4,044 in Philadelphia in 1793; and 5,823 in the other three years) *aegypti* must have been fairly well scattered over the town at the commencement of hot weather. That is, they must have hibernated as eggs from the previous fall. The epidemic of 1820 in Philadelphia, on the contrary, was confined absolutely to a small part of the water-front. Cases—and many of them—contracted in the infected district and developing in many other parts of the town, in no instance led to secondary cases—to the great satisfaction of the non-contagionists. Evidently, in 1793 *aegypti* were all over Philadelphia, and in 1820 were confined to a small water-front district. This last epidemic may, then, have been caused by insects, and the progeny of insects, introduced by ships earlier in that season. The first cases were reported July 24.

It is to be noted that water in pipes was introduced on the water-front in 1821 and 1822 (personal letter of C. E. Davis, Chief of Water Bureau, Philadelphia, April 4, 1922) which doubtless did away with fire barrels and other containers on the wharves which had furnished breeding places for *aegypti* going ashore from vessels lying alongside. Since that date only one outbreak of yellow fever, that of 1853, limited to a few squares on the water-front, has occurred in Philadelphia.



Naturally, if the mosquitoes introduced be already infected, they may furnish not only the parasites necessary to infect, but the parentage of the insects necessary for subsequent conveyance. They could, however, for the reasons given, rarely produce more than a very localized epidemic, disappearing with cold weather.

(3) *The conditions of temperature may be such that when aegypti are introduced they cannot propagate.*

The mosquitoes introduced, however, may be able to feed to a small extent, and, if infected, to infect a few men.

Infection of men by *aegypti*, already infected, leaving a vessel, would explain the infection reported at Quebec, in 1864, of a man on the dock at which a vessel from the West Indies lay; at St. Nazaire, in 1861 (the famous epidemic of the *Anne-Marie* so beautifully reported by Mélier 1863), in which fifteen cases are reported in men who had not been aboard the infected vessel; at Swansea in 1865 (the report of which by Buchanan, 1866, rivals that of Mélier) to which twenty-two cases and fifteen deaths are positively, and twenty-nine cases and sixteen deaths are probably, ascribed. In none of these outbreaks did secondary cases occur.

If propagation be prevented by absence of suitable breeding places, there will be no infestation of *aegypti*, although those introduced will feed as long as they remain active and, if infective, may give rise to cases of yellow fever. From these, however, we would not have secondary cases, except in very rare instances from the infection of uninfected insects introduced at the same time. A number of the "sporadic cases" reported in Philadelphia along the water-front after 1822 were probably from this cause. The writer was able, in August, 1903, to raise *aegypti* from cans exposed on the fruit wharves in Baltimore and from no other places in town. Had there been suitable breeding places in sufficient numbers there probably would have been a local infestation of *aegypti*. That it was not a question of temperature was shown by their breeding where artificial containers were provided.

(4) *Aegypti may be active aboard ship in the harbors of ports, the temperatures of which ashore are well outside of the feeding limits of that insect.*

This is in no wise uncommon. They remain active aboard in the warm or heated compartments of the vessel and, if infected, may communicate yellow fever to the seamen aboard or to men coming aboard, who may develop it ashore. This has occurred at Brest and Rochefort, in France; at Portland, Maine; at Halifax and Boston, more than once; and at St. Pierre de Miquélon. It is not, properly speaking, an introduc-

tion of *aegypti* into a place and is, naturally, of no epidemiological, and of little sanitary, importance.

Naturally, places under the temperature conditions of (3) or (4) are always "non-infectible territory." Those under (2) are uninfected also until *aegypti* are introduced and have become sufficiently abundant to propagate yellow fever, as are those under (1) during the winter.

The terms were coined before the rôle of the *aegypti* was known, to indicate the well-known fact that yellow fever would spread in certain places if men with yellow fever were introduced, while in others it would not.<sup>4</sup>

#### ZONES OF OCCURRENCE OF YELLOW FEVER

The regions in which yellow fever occurs may be divided into:

1. *Epidemic areas.* In these the duration of yellow fever is limited by the season—as at Memphis, Tennessee; Montgomery, Alabama; etc. *Aegypti* are present and active during only a part of the year; not active during the cold season—existing then, if at all, only in the egg stage.

Epidemic areas are naturally divided into two classes: (a) those in which *aegypti* exist permanently—the eggs surviving the winters—and (b) those in which the species does not so survive.

In the first, the places are always "infectible territory" in the summer—as Mobile and Memphis. In the second, they are infectible only when a new supply of *aegypti* is introduced—as were Boston and Newburyport, Massachusetts, etc. There are, naturally, a certain number of border-line places in which the eggs survive some winters, but not all. These places are infectible in some summers without a new introduction of *aegypti*; and in others such an introduction, generally by vessel, is required for them to become infectible. Such places in the past were Philadelphia, Baltimore, and (rarely indeed) New York.

2. *Endemic areas.* In these the duration of yellow fever is not limited by the season. *Aegypti* are present in sufficient numbers and functionally active throughout the entire year, and once the fever is established, its duration, in the absence of control, is dependent on the supply of susceptible men. It may last for several years, as at Tampico, Cartagena, Maracaibo, etc., eventually dying out, not for lack of active

<sup>4</sup> The writer, in the summer of 1899, suggested to Dr. Finlay the examination of both kinds of places for his *Culex icteroides* (*Aedes* (*Stegomyia*) *aegypti*) as a test, partial, of course, of his (Finlay's) doctrine. This was begun at Las Animas, but was interrupted by the writer's leaving Cuba to work in the Key West epidemic of that year.



insect vectors, but from failure of the human host. Such regions may be called *temporary endemic* areas. If the conditions of population are such as to maintain a sufficient supply of susceptible men, we have a *permanent endemic* area, where the disease not only lasts through the whole year but does not tend to die out spontaneously. A permanent endemic area may be a true *permanent endemic center*, comprising a single large city. Such were Havana, Rio, and Guayaquil. Or it may be a *regional focus*, such as that of Yucatan and Campeche.

The difference between epidemic and endemic areas, as we have defined them, depends on the biology of *Aedes aegypti*; that between temporarily endemic and permanently endemic areas is sociological, and depends on the supply of susceptible men.

The difference in epidemiology which yellow fever presented in different places naturally led, years ago, to the classification of such regions into different zones for its occurrence. These have been formulated by a number of men: James Jones (1858), Bérenger-Féraud (1890), Guiterras (1888), and others eminent in the study of this disease. That formulated above, while different from any of the classical divisions is, the writer believes, correct, and as comprehensive and definite as is consistent with reasonable brevity.

The word "endemic," applied to a disease, as used in medical literature, carries the meaning of permanence in a community or region, although there is no etymological reason therefor. Liebermeister's differential definition (quoted by Chaillé, 1882, p. 91) is: "An endemic is limited locally and remains long or persistently, while an epidemic appears at intervals and again disappears." The definition of an endemic disease quoted from an unnamed French writer by the British West African Yellow Fever Commission (Yellow Fever Commission (West Africa), 1916, p. 250): "*Une maladie habituelle du pays*," is, one thinks, as definitive as the German's and shorter.

Hitherto, yellow fever has always been regarded as "endemic" in both classes of places—those where it persisted indefinitely and those where it died out after a period of several years. Neither of the qualifying terms "temporary" or "permanent" is altogether consistent with the current use of the word "endemic." Nevertheless, the difference between (1) places in which yellow fever will last two or three or even more years and yet die out from lack of susceptible men, and (2) those places in which, enough of such men being present, it lasts indefinitely, is a real and important difference, and should be provided for in place designations.





PART II

DISEASES WHICH HAVE, OR MIGHT HAVE,  
BEEN CONFUSED WITH YELLOW FEVER  
IN THE PAST





## PART II

### DISEASES WHICH HAVE, OR MIGHT HAVE, BEEN CONFUSED WITH YELLOW FEVER IN THE PAST

In looking over past history one finds accounts of the existence of diseases and of epidemics. Not a few of these—since 1492 especially, and some before—have been considered by medical historians to have been yellow fever. The earlier accounts, up to near the end of the seventeenth century, are none of them by physicians,<sup>1</sup> and even when a description is given one is rarely able by it to recognize the malady with any certainty. Quite frequently, the description evidently covers several diseases considered by the chronicler as different manifestations of one, with, at times, additions as evidently imaginary. The taste for the grotesque and the horrible seems ingrained in men of a certain stage of culture. More often, no description is attempted; the prevailing epidemic is simply classed as “*una peste*,” “*una fiebre pestilencial*” etc. These are the most common terms used, although “*la modorra*,” (the lethargy, the stupor, the sleepiness), meaning, I presume, an ultimate comatose state, or it may be simply weakness, was common in early Spanish writing, and evidently covered more than one disease. “*El contágio*” is also used, implying the opinion that the disease in question was communicable, and “*la epidemia*,” implying its high prevalence.

Now doubtless some of these epidemics were yellow fever and, as undoubtedly, some of them were not; and to judge between them with even an approach to accuracy, it seems advisable to consider what epidemics besides yellow fever one might expect to find in the countries and at the times covered by these accounts, and also what would be the points of difference between them, either in occurrence, or in such symptomatology as would be apt to be noted by the chroniclers of that day, premising that the symptoms must be quite obvious to be so noted.

<sup>1</sup> Dr. Chanca, the physician to the second voyage of Columbus, indeed mentions in his letter (Monte y Tejada, 1853, p. 298) relating to the voyage, “one-third of our people have fallen sick within four or five days” (the implication is after landing), but beyond ascribing it to “the toil and privation of the journey,” and the pious “trust in our Lord to be able to restore all the sick to health,” he gives no account of the disease. Besides this, the first account by any physician of any epidemic in the New World that I am able to find is that of the “Fever of Olinda” (Pernambuco), in 1685 and 1686, by Dr. João Ferreira da Rosa (1694), 200 years later.

## DISTINCTIVE CHARACTERISTICS OF YELLOW FEVER

Before going into the differentiation of yellow fever from the diseases with which it might have been confused, it may be well to give a very brief résumé of such characteristics of yellow fever itself as are likely to be noted by our old chroniclers or which may be inferred from the context of their accounts and which are significant for distinguishing other diseases.

(1) It occurs (in epidemics) only in hot, or fairly hot, weather.

(2) It is apt to occur in epidemics: certain to do so in a crowd of white (susceptible) newcomers.

(3) It is a disease of collections of men—civic rather than rural.

(4) It not only develops, but is contracted and propagated, aboard ships on the high seas, and under some conditions is communicable from ships to places ashore and *vice versa*.

(5) It is a disease of high, or fairly high, case-mortality among all races except negroes, and especially among robust adults. Among negroes the death rate is quite low absolutely; very low comparatively.

(6) The deaths are usually from the fifth to the eighth day, grouped on the sixth and seventh.

(7) Convalescence, once established, is *complete* and rapid. Anemia is not produced by yellow fever, or is found only, to quote Pym (1815), in the fortunate “few who survive venesection.”

(8) The characteristic *symptoms* which would be noticed are: sudden attack; severe pains in head, back, and limbs; *extreme* prostration; jaundice; hemorrhage—especially showing at, not necessarily from, the mouth and nose; and black vomit.

Of these symptoms, sudden attack and the pains mentioned are too common to other diseases to be distinctive.

Jaundice, too, is, of course, a symptom common to many diseases of hot climates, although it is not confined to those of such climates. Of yellow fever, “the dead are always yellow,” says Dutroulau (1868, p. 409).

Hemorrhages, not unlike those in yellow fever, occur in certain epidemics of typhus and spirochetoses.

Black vomit, which is merely blood altered by the acid contents of the stomach, may, of course, occur among the hemorrhagic manifestations of the spirochetoses, although in fact the gastric hemorrhage, when it occurs in them, has usually been so profuse, or the gastric acid in so small an amount, that it has usually been reported by the older writers not as



black vomit, but as blood. In the cases of Graves and Stokes (1827)—trained clinicians—true black vomit was reported, and so in those of Noc and Nogue (1921). In no other epidemic disease except yellow fever and certain spirochetoses does this symptom occur at all frequently. If the spirochetosis be louse-borne (like typhus) it will not continue in a hot climate.

Black vomit, the lesser degrees of it at any rate, is also occasionally produced by prolonged straining vomiting, as in sea-sickness, after ether, etc.; and full black vomit occurs in some cases of intestinal obstruction and peritonitis, and from the ingestion of irritant poisons, alcoholism, etc. The occurrence of isolated cases of black vomit, then, is not sufficient evidence of yellow fever.

On the negative side, that is, the non-report of this symptom, one must note that not a few of the earlier writers who lay stress on hemorrhage from the mouth in yellow fever do not mention black vomit. They either fail to notice it, or fail to distinguish it from the very dark green (“bilious”) vomit so common in malarial fevers. Towne (1726), a physician, who wrote an otherwise extremely good description of yellow fever in Barbados, states that the vomit “is of the bilious kind.” Père du Tertre (1667, vol. i, p. 423) mentions “continual vomiting,” but not black vomit, in the “*peste*” of 1648 of Guadeloupe, which is generally accepted as being yellow fever. The “*mal de Siam*” of 1690 and later, in Martinique, was certainly yellow fever. Père Labat (1722, vol. i, page 73), who was on the island from 1694 to 1707, in his general description of it, speaks of hemorrhages—“the overflowing of blood from all the orifices of the body, even by the pores”—and excessive vomiting, even “of worms,” but does not specifically mention black vomit.<sup>2</sup>

It is not probable that the good father would have recognized black vomit as being blood. Its appearance does not at all suggest blood, unless, of course, in those extremely rare—and always fatal—cases in which the hemorrhage is so profuse that the vomit is of *red* unaltered blood. Indeed, it was not until 1738 that Henry Warren showed that black vomit was blood, altered by the gastric juices.<sup>3</sup> Even in 1806

<sup>2</sup> In his own case (vol. i, p. 436) the “much blood from” (his) “mouth” which showed early on the second day of his illness, must have been a hemorrhage from the posterior nares and naso-pharynx. Much vomiting occurred later, so that he could not take the sacrament, but no more is heard of the hemorrhage and nothing of black vomit, and his physician did not regard him as dangerously ill. A large gastric hemorrhage on the second day had been death.

<sup>3</sup> “I ought to observe here, that the fatal black Stool and Vomiting are vulgarly supposed to be only large Quantities of black Bile or Choler; which false

and 1807 "the learned Bancroft" (1811), in his *Gulstonian Lectures* of those years, deems it necessary to argue *in extenso* for this black vomit being gastric hemorrhage; and James Johnson (1826, p. 287) is only "inclined" to accept this view, which "nearly coincides with that of other accurate observers." Its nature was evidently not considered beyond dispute even then.

On the other hand, the Spanish Americans knew yellow fever as "*vómito prieto*"—black vomit—long before this symptom was given prominence by the English. Indeed, a statement as to the nature of the vomit, similar to that made by Warren, appears in the description of the "*peste*" of 1648 in Yucatan—"there followed some vomits as of rotted blood."<sup>4</sup> This "*peste*" was certainly yellow fever.

For the positive side of this question, however, we can say that a fever epidemic in a hot climate in which either of these two symptoms, hemorrhage from about the mouth (always excluding scurvy) or black vomit, the latter especially, was noted as of fairly frequent occurrence, must, in the present state of our knowledge, be considered as having been either yellow fever or one of the tick-borne spirochetoses, and very generally the former. The writer knows of but two instances of such an epidemic being other than yellow fever; one in Senegal, reported by Noc and Nogue (1921) and one in Barbados, investigated by Guiteras in 1916 (see Guiteras, 1917), which probably was also a spirochetosis. Both showed a greater mortality among negroes than whites, and a longer survival of fatal cases than is normal for yellow fever. There may have been others of the same kind, however, not recognized.

Black vomit, under such conditions, in the Americas at least, is well-nigh pathognomonic of yellow fever. The Spanish Americans early crystallized this idea in the name of the disease, "*vómito prieto*"; and Sir William Pym laid stress on it above all things. To *him* black vomit *was yellow fever*; and hedging a very little, and expressing what I think he really intended, we may say that an epidemic of fever in which black vomit is at all common is, in tropical America, almost certainly yellow fever—but for the Barbados fever of 1916 and the possibility of tick-borne spirochetoses, one would strike out the "almost."

---

Notion seems to be owing to that fixed unhappy Prejudice, that the Fever is purely *Bilious*. But let any one only dip in a Bit of white Linen Cloth, he will be soon undeceived, and convinced that scarce anything but mortified Blood is then voided, for the Cloth will appear tinged with a deep bloody Red or Purple, of which I have made many Experiments" (Warren, 1740, pp. 39–40).

<sup>4</sup> "Seguíanse unos vómitos de sangre como podrida" (Lopez de Cogolludo, 1688, *lib. xii, cap. 14*).



If this epidemic gives a much lower mortality among negroes than among other races, if it be decidedly a civic rather than a rural disease, if death is usually from the fifth to the seventh day, and second attacks do not occur, the evidence for its being yellow fever seems impregnable, although it was strong enough before.

#### DISEASES TO BE DISTINGUISHED FROM YELLOW FEVER

Bearing in mind the kind of records from which we must seek our information for the period with which we are chiefly concerned (to the middle of the seventeenth century) it is necessary to be at some pains to differentiate yellow fever in those records from various diseases which, in more recent literature, would hardly come into consideration for differential diagnosis. In fact, it is necessary at least to bring into the question such diseases as smallpox and measles, which would seem, from the modern viewpoint, to have almost nothing in common with yellow fever, except the likelihood of occurring in fatal epidemics.

##### *Smallpox*

Introduced into America by the Europeans, smallpox was a scourge to its native races, far less so to the Europeans, most of whom were immune by previous attack. Naturally, when any description of the disease itself is given, no matter by whom made, there would be no possibility of mistaking smallpox for yellow fever, or the reverse.<sup>5</sup>

##### *Measles*

What is said of smallpox applies to measles, to a less degree maybe. It was less certain of recognition, less deadly to the Indians, and negligible in its effects on Europeans. While not by any means the scourge that smallpox was, measles was quite fatal to the American natives, even in the tropics. Where it was cold, it was to them a deadly pestilence. When one hears, then, of fatal epidemics among the Indians, or any primitive race, following their contact with Europeans, one must—unless the description of the disease negative them—consider smallpox and measles as possibilities. Where there is no description, and the

<sup>5</sup> The picture writings of the Aztecs (*Codex Aubin*, p. 85 and *Codex Telleriano-Remensis*, in many places) plainly show the terrible epidemic of smallpox introduced into Mexico in 1520 by a negro sick of that disease, in the train of Narvaez, and which did much to break the resistance to Cortez. It was known as “great-fire” (“*Nohkakil*”) by the Mayas after it came to them, and even the far less cultured tribes in North America soon learned, to their sorrow, to know it well.

disease does not seriously affect, and is not noticed by, the Europeans who are in contact with it, the evidence for its being one or the other is strengthened, although typhus or influenza are not to be excluded.

Such was "*la modorra*" ("the lethargy") which destroyed a good part of the Guanches of Teneriffe in 1494 after the battle of Laguna. According to Espinosa (trans., 1907, p. 105) the Spaniards went freely into the stricken territory, bringing back some captives—"owing to the pestilence, our people did not meet much resistance"—and are not noted as suffering any sickness, except from the roughness of the country and the scarcity of provisions, nor as being concerned about that among the Guanches. Finlay (1904) suggests this epidemic as being yellow fever, and as such it is accepted by Augustin (1909, p. 180). No description is given of this disease, but from its epidemiology it could not have been yellow fever, and either smallpox, typhus, influenza, or measles—all four prevalent in Spain about this time—seems the most probable diagnosis. One of the first two may seem necessary to account for the heavy mortality, and there had been a severe epidemic of typhus in Spain in 1489 and 1490 at the siege of Granada. Yet its rapid progress<sup>6</sup> and the fact that the chronicler does not mention that it was smallpox and that the Spaniards paid absolutely no attention to it seem, to the writer, to make measles at least probable. Measles, in a virgin race in a mountain climate, and aided by the privations following an unsuccessful war, might well make a heavy mortality.

### *Influenza*

This disease is recorded as epidemic in Europe, in France, Germany, or England, quite frequently, certainly six or eight times from 1403 to 1600. In Spain, Villalba (1803) reports it epidemic as "*el catarro*" in 1580 and many times thereafter. Judging from our recent experience, it would have had a good chance to cross to North America from the northern countries—by Breton, Scandinavian, and British fishermen, especially—much less, to reach the lower latitudes in the yellow fever zone.

The writer is sure that no epidemic of influenza in Europe was reported as yellow fever, nor was the contrary mistake made. The same seems to have been true also for tropical America in early times. Yet influenza must be considered as a probability in the great epidemic which occurred among the Indians on the New England coast about 1618 and which was

<sup>6</sup> "A woman of the island announced the pestilence. . . . asking why they did not come up and occupy the land, for there was no one to fight, no one to fear, all being dead" (Espinosa, Markham's translation, p. 104-105).



the salvation of the Plymouth colonists. Noah Webster (1799, vol. 1, pp. 176–179) gives a detailed account of this and considers it to have been yellow fever. Since it occurred during the winter, this disease need not be considered, spite of the statement of his Indian informants that the sick were as yellow as the piece of yellow cloth he showed them. It could well have been influenza, being in the part of America into which it would have been most readily introduced. Measles is another tenable, but perhaps less probable, supposition. Smallpox, Webster had specifically excluded.<sup>7</sup>

Whether “*el bicho*” (“the worm”), epidemic at Lima in 1781 according to Leblond (1805), was yellow fever, as reported by him, or grippe, is uncertain. The evidence is not definitely determinative, but to the writer renders the diagnosis of yellow fever much the more probable.

The very widespread epidemic on the Andean plateau in 1759, which gave rise to the report of yellow fever in Quito, was, the writer believes, influenza. Why it should ever have been considered yellow fever it is hard to say, as its first appearance in the highlands of Peru was “in the time of ices” (Ulloa, 1772, p. 200).

Clinically, the two diseases, seen as epidemics, are sufficiently distinct, yet the writer investigated, in 1920, two small epidemics of grippe in Peru reported as yellow fever, and in 1921 a larger one of yellow fever, reported (among other things) as grippe “*à la forma gástrica*.” In Louisiana, in 1918, a number of physicians familiar with yellow fever regarded the influenza epidemic with suspicion, and a few were near reporting it as yellow fever. To the writer, there seems no reason to mistake them clinically—seen in fair sized groups.

### *Dysentery*

With a clinical description of either disease, dysentery and yellow fever would never be confounded. Yet in accounts of epidemics among English troops and, to a less degree, colonists, in America, “fevers and fluxes” are frequently grouped in both the morbidity and mortality re-

<sup>7</sup> The experiences of the writer with the American aborigines, both in investigating an epidemic, which had occurred among the San Blas Indians, suspected of having been yellow fever, and at other times, is that they, when really friendly, carry their courtesy so far that one is pretty apt to get the information *they think* he wants, let the facts be as they may. We were assured by the San Blas that black vomit had been frequent and abundant; that most deaths were in convulsions—dramatically illustrated; that the eyes of the sick and dead were as yellow as the gold coin exhibited. Yet from epidemiological evidence and the examination of three convalescents, we were able, positively and without any doubt, to determine this epidemic as having been grippe.

ports. Dysentery, too, seems to have been one of the diseases which, with syphilis and fevers, was frequently grouped as one morbid entity by the early American historians. Obviously, then, when we simply hear of a great sickness—especially in an army—in the tropics, with no description of its nature, the chance of dysentery's being a factor in it is to be considered.

*Plague, bubonic or pneumonic*

During the time with which we are concerned, plague was always present in the Levant, and from time to time prevailed as extensive and deadly epidemics in most parts of Europe and North Africa. By these characteristics, then, it could not be distinguished from yellow fever. It can occur, however, in cold weather as well as in hot, and is far less apt to show as a ship disease.

The symptomology of plague is different enough from that of yellow fever, and one might expect excessive vomiting, hemorrhages by the mouth, and jaundice to be noted in yellow fever, and the bubo in "*peste*."<sup>8</sup> This latter was well known as a sign of "*peste*" to both laity and physicians in Europe. Where the nosology, however, included syphilis as well as some other diseases under one head—"*la epidémia*," "*la enfermedad*"—as was the case with that of all early epidemics in America—the presence of buboes need not imply "*peste*."

As "*peste*" and typhus, held to be "the same in essence," were, in the sixteenth and seventeenth centuries, regarded as the types of "pestilential," that is, communicable, diseases, their characteristic marks, buboes and petechial spots, were invariably sought for whenever there was suspicion that the disease occurring might be of such a character; and the occurrence of either was considered strong evidence of its so being. Not only were typhus and plague held to be the same, but the general teaching was that all fevers were identical in nature, differing only in degree and the temperament of the sufferer.<sup>9</sup>

<sup>8</sup> Yet Henry Warren—a physician of excellent reputation—writing in 1740, regards "this malignant fever," afterwards called "yellow fever" by Hughes (1750), as the Levantine plague, and states that it was brought to Barbados about 1723 by the British man-of-war, *Lynn*, from Martinique, and again in 1733 was conveyed from Martinique, where it had been introduced in baled goods by a Marseilles vessel, from St. Jean d' Acre. Ligon (1673, p. 21), too, a layman, states that "the Plague, or as killing a disease," was prevalent in Barbados in 1647 when he landed there. Whether this was yellow fever we do not know, but it may well have been.

<sup>9</sup> Arejula, even in 1806, is just bold enough to differentiate between "*peste*," "*epidémia*," and "*contágio*"; and he cautiously suggests that they are "as it were"



We have said that "*peste*" is far less apt to show as a ship disease than yellow fever, and, indeed, their behavior aboard ship should furnish valid points for differentiation between these two diseases. If in the pneumonic form, "*peste*" aboard ship would be apt to occur as an epidemic (as would yellow fever if there were sufficient active *aegypti* aboard) and with a case mortality of near or quite 100 per cent. The mortality of yellow fever, although high, is, even aboard ship, less than that of pneumonic plague. Such an outbreak of plague would be quickly over, however, and would be more apt to begin in cold climates than in hot. A considerable number of cases of bubonic plague might be caused aboard ship by the presence of infected rats, and more, probably, when the forecabin was below deck, as in old times, than in present-day vessels. Even then, however, one would scarcely think that an epidemic could be thus caused, certainly not such an explosion as sometimes happens with yellow fever. Under such conditions, bubonic plague might well be long continued aboard ship, even longer than yellow fever, and give a high mortality, equal eventually to that of severe yellow fever, but less than that of the pneumonic form of "*peste*."

Considering the close association of men aboard ship, it does not seem reasonable that any considerable number of her personnel could develop bubonic plague without the existence of the buboes being noted: they would certainly be looked for, and, being painful and tender, would be easy to find. For the pneumonic form, one must depend on the epidemiological picture—the short, excessively virulent epidemic, beginning very soon after leaving an infected port, and, in general, in a cold climate.

That "*peste*" is rare aboard ships in modern times is evidenced by the fact that during the present pandemic, now nearly thirty years old, there has been reported, the writer thinks, no *epidemic* of either pneumonic or bubonic plague aboard ships on the high seas, although some vessels have shown scattered cases of the latter form aboard, and a large number have carried infected rats. Nearly all of the ports infected during this pandemic have been in warm or temperate climates.

Quite certainly there was never plague in America before 1898, and the Levantine "*peste*" which Warren claims was brought to Martinique and later to Barbados from Martinique in 1733, was as certainly yellow fever.

There is to me no question but that the epidemic of 1694 at Rochefort—a very celebrated one, as leading to the first maritime quarantine

---

species of the same genus, although "the teaching of the schools is that they differ only by the existence in greater or less degree of the conditions producing them."

against yellow fever from the French Antilles—reported by Chirac (1742) and accepted by La Roche (1855, vol. i, p. 48) and by Audouard (1824) as yellow fever, was plague. Chirac speaks of axillary and parotid buboes as common, although fewer in the groins. It seems rather strange that the earliest protective measures against the introduction of yellow fever should have been taken under a misapprehension.

*Relapsing fever ("famine fever") and spirochetal diseases generally*

These are insect-borne diseases, lice or ticks being the vectors. "Famine fever," louse-borne, was a scourge in the Middle Ages and up to the early part of the nineteenth century. It occurred in widespread epidemics with the misery following wars, and with a high mortality among the half starved people. Todd (1922) gives the case fatality as 60 per cent among Rumanian prisoners during the Great War, and 80 per cent in India, where the native treatment is strict diet. Lind (1792, p. 188), on the authority of Sir John Elliott, gives the mortality at Naples in 1764 as 200,000 out of 2,000,000 population—doubtless an estimate only, but undoubtedly of a severe epidemic, and which probably was mixed with typhus.

The diseases of this group occur both in cold and in hot weather, lice naturally thriving best in the former and ticks in the latter.<sup>10</sup>

The tick-borne infection of *Spironema duttoni* seems to be in general decidedly milder than that of the louse-borne *S. obermeieri* (Gouzien, 1924). Brumpt (1922, pp. 53–56) states that each of these organisms has its own vector, that the organism of tick-fever is not conveyed by lice, and that experimental conveyance of louse-borne fever by ticks is doubtful. This implies that the parasites of the two forms are essentially different. Todd (1922), however, classes them both as *S. recurrentis*, and prefers to describe "the relapsing fevers as one disease which presents unimportant local modifications."

The symptoms in some epidemics may be quite like those of yellow fever: profound jaundice; hemorrhages, including gastric hemorrhages, and hence black vomit. And, unless the occurrence of relapses was noted, or that death came less quickly in such cases, no difference in symptoms from those of yellow fever would be apt to be recorded by lay historians, or even by physicians. Unless, then, they occurred in cold weather, it might not be easy for us to differentiate such epidemics from those of yellow fever. A diagnostic differential in epidemics among a mixed

<sup>10</sup> Relapsing fever (lice-borne) is prevalent in the Punjab, but only in the cold season (Cragg, 1922).



population of whites and negroes would be that relapsing fever is as fatal among the latter as the former. It is also less apt to be confined to towns and cities—that is, may be a rural as well as an urban disease, which is less usual with yellow fever.

The fever which La Roche (1855, vol. i, p. 582) mentions on the authority of Arrott, as occurring at Dundee in 1843 and as presenting “on some points, a resemblance to yellow fever,” and the small epidemic of 21 cases and 17 deaths in the winter of 1826 and 1827 at Dublin, were quite certainly spirochetal infections—probably “famine fever.” Yet both Graves (1848, p. 301) and Stokes (1876, pp. 110–111), clinicians of the first order, but unfamiliar with yellow fever, considered the latter as “the yellow fever of the West Indies.” Arrott (1844, p. 133) says: “The similarity of the symptoms during life and of the morbid appearances observed after death, so nearly agree with the descriptions of yellow fever of the West Indies, and with the minute accounts of the Gibraltar epidemic of 1828, given by Louis, as to leave little doubt on my mind that the only difference between these diseases and the Dundee epidemic, if difference there be, is a difference in degree and not in kind.”

Of the same fever in Edinburgh, Cormack (1843) writes that it corresponded closely with the description of yellow fever of the West Indies as given by Sir Gilbert Blane, a well known writer on yellow fever, of which he had seen much in the West Indies.

Neither Craigie (1843) nor David Smith (1844), who reported epidemics in 1843 in Edinburgh and Leith, regarded it as “the yellow fever of the West Indies.” The mortality was not high enough and “many patients showed no vomiting of blood.” This was clearly relapsing fever. Craigie gives 110 relapses out of 182 cases after the patients had been up and about for four to six days. The first cases observed in Edinburgh were in March.

For an epidemic occurring in a hot, tropical country, and hence quite surely “tick fever,” Cornilliac (1873, p. 375) quotes McAuliffe concerning an epidemic of relapsing fever at Reunion (date not given): “I even believe that it would be impossible to clearly distinguish these affections” (relapsing and yellow fever) “if both raged at the same time in a community.” Both seem to have occurred simultaneously, or nearly so, in the district about Bucaramanga, in Colombia, in the summer and fall of 1923.

The epidemic at Quebec, reported as yellow fever by Walsh in 1806, and the subject of considerable controversy, was clinically a spirochetosis (it is well described), but possibly not from the organism of Ober-

meier. The recent epidemic of spirochetosis in Senegal, reported by Noc and Nogue (1921) was, except for its occurrence in a hot country, more like the one in Dundee. It showed black vomit, which the Quebec epidemic did not; but relapses and a greater mortality among blacks than whites.

### *Typhus fever*

This, also an insect-borne disease, was more widely distributed even than plague in the times that we are considering and, especially when epidemic, had a high mortality. What it can do under unfavorable sanitary conditions even in modern times is shown by the Serbian outbreak in 1914–1915. Its vector being the body-louse—possibly other man-infesting lice as well—naturally it occurs by preference in cold weather. It is also less essentially a civic disease than yellow fever.

The symptomologies of the two diseases, yellow fever and typhus, would seem to be very different, almost contrasting, yet they were regarded as the same, or at least as kindred diseases, “modified by climate and marine conditions,” by many of the leaders of the medical profession in Europe, most of whom had little experience with yellow fever. Even as late as 1866, Anstie (1866, p. 86) writes that from “the histories of West Indian epidemics” he believes that “the disease” (yellow fever) “has strong affinities with our English typhus . . . that were these fevers” (typhus and relapsing fevers) “transplanted to the . . . tropical regions . . . they would develop these features on which so much stress is laid” (jaundice and black vomit) “with constancy, instead of occasionally, and would then be indistinguishable from the yellow fever.” Stress was laid on the “essential similarity” between the petechial spots, considered pathognomonic of typhus, and the livid spots (indeed from a similar mechanism) in yellow fever. These petechial spots, as well as buboes, were very generally sought for when there was a question of an illness being “pestilential,” i.e., contagious. “American typhus” and “*typhus icteroides*” were designations for yellow fever for many years. Indeed, the latter is still common in the mortality records in Spanish America.

The differential here is unusually clear. Typhus never continues as an epidemic in a hot climate nor yellow fever in a cold one. We may therefore exclude typhus from our consideration of epidemic diseases in the endemic yellow fever zone of America. In the Old World, however, the case is different, and typhus may well have been reported as yellow fever in Europe. It was certainly typhus and not yellow fever which



ravaged Catalonia in 1764 and later, as described by Masdevall (1786, p. 18 *et seq.*).

Whether the fever of 1822 in Paris, with jaundice, black vomit, and petechiae, was typhus may be a question. Lassis (quoted by Augustin, p. 392) and Robert (1826, vol. i, p. 104), who was familiar with yellow fever, reported it as yellow fever; and Magendie (1842, vol. 1, p. 117), who mentioned 11 cases in hospital, confirmed the existence of these symptoms. It was almost certainly not yellow fever. The case mortality was too low and the patients were sick too long. It was more probably some spirochetal infection rather than typhus. The single autopsy (Andral's) reported by La Roche (vol. i, pp. 272–273) may have been typhoid. This probably is the basis of La Roche's opinion that the epidemic was of "typhoid fever, modified by the extreme heat of the season."

Villalba (1803, vol. i, p. 112) states that typhus, epidemic in Spain since about 1557, had been introduced into Mexico in or before 1570 and made great ravages in the City of Mexico, where it was described under the name "*tabardete*" by Dr. Francisco Bravo.

Was the famed "*matlazahuatl*," accredited with such ravages on the Mexican Plateau in 1545, 1576, 1736, 1737, 1761, and 1762, the typhus, introduced from Spain, or was it some indigenous disease, existing from pre-Columbian times? The general belief is the latter. This the writer is inclined to doubt. It is true that one of these epidemics is recorded in Mexico in 1545, which is prior to the recognized introduction of "*el tabardillo*" from Spain and, indeed, prior to the date (1557) of the mother epidemic in that country. Yet, as this disease had existed in Spain at the siege of Granada in 1489–90—brought by soldiers from Cyprus—and appeared again soon after the Civil Wars (1518–20) it could have been introduced into Mexico earlier than Villalba reports. We are quite sure that it was introduced in 1526. This whole matter will be considered later in the discussion of the "Place of Origin of Yellow Fever." It is enough to say here:

(1) In spite of the hemorrhagic symptoms accredited to it, "*matlazahuatl*" was not yellow fever, as it prevailed on the high plateau, where yellow fever has never been known to spread, and, in 1576 at least, it occurred during a period of unusual cold, even for that altitude. There is no account of it in the *tierra caliente*.

(2) The epidemic of 1576 was called "*tabardillo*" by Mendieta (p. 515), a contemporary historian.

(3) The typhus now existing in the Mexican highlands is known as

"*el tabardillo*," and seems to be identical with that in Europe and North Africa; and, save an occasional epidemic of smallpox, we know of no other very fatal epidemic disease there.

Several epidemics of a fever of very high mortality, characterized by blood from the mouth and nose, and by jaundice, occurred also in the Andean plateau, from Potosí and Cerro de Pasco to Quito and Popayan, in 1720 and subsequently. By some Spanish writers, these also were called "*matlazahuatl*," this name quite certainly being copied from that given similar epidemics in Mexico, of which they had heard. The very widespread and fatal epidemic of 1759, noted, and to an extent, described by Antonio de Ulloa (1772, p. 200 *et seq.*), was, in the opinion of the writer, quite certainly one of influenzal pneumonia; but in other epidemics petechial eruptions were noted, and the conditions under which they occurred and their epidemiology leave no doubt of their being typhus. Some of the others may have been relapsing fever, or even the two occurring simultaneously.

It is these epidemics that gave rise to the statement, so often ascribed to Humboldt, that yellow fever had prevailed as an epidemic in Quito. I cannot find that he ever made it. Certainly even the Andean foothills in Peru were free from *aegypti* in 1920-21.<sup>11</sup>

### *Scurvy*

This could scarcely occur in the tropics except aboard ships, and yet might enormously increase the disability, and indeed the mortality, of even mild diseases in men just landed with it. It is doubtless this which Dr. Chanca (Monte y Tejada, 1853, p. 298) had in mind when he instanced the bad voyage—"la mala pasada del camino"—as being a cause of the sickness occurring in the second expedition of Columbus when they landed in 1493 on Hispaniola; and he was in all probability right, no matter what other causes may also have been active. The same may well have been a principal factor in the sickness, associated with want and privation, in the Dutch garrison at Valdivia (Chile) in 1643, which Bérenger-Féraud counts as probably yellow fever—why, one does not know. They had just completed a very long and arduous voyage and had landed on a desolate, barren coast. On the other hand, scurvy would never last any length of time ashore in the tropics of

<sup>11</sup> Some of the Mexican epidemics may well have been mixed ones, of typhus and relapsing fever, or some other spirochetosis, such as is now (1922) prevailing in Poland and the Ukraine. They have the same vector. The whole subject will, however, be discussed later.



America (Valdivia is in lat. 40° S.): there is too much vegetation available.

Scurvy was little, if at all, known as a ship disease prior to the discovery of America: prolonged voyages were rare before that time. Villalba (1803, vol. i, p. 76) quotes from João de Barros, an early Portuguese writer, a description of a severe outbreak of scurvy aboard the squadron of Vasco da Gama, after passing the Cape of Good Hope in 1498. The narrator evidently regards it as a hitherto unknown disease, and Villalba thinks the same. It soon, however, became too well known aboard ship to be there mistaken for anything else; but when we have no account of the *nature* of a sickness which continues to prevail aboard ship on a long voyage, one naturally thinks first of scurvy. Thus, the loss of 54 out of 70 colonists for St. Christophe (from Brest) aboard the *Cardinal* before they landed, and "the most of the others half dead" (du Tertre, 1667, vol. i, p. 15), implies scurvy.

It may be noted here that the dead of scurvy in some points resemble those dead of yellow fever, and that hemorrhages from the gums and into the cutaneous connective tissue are common in both scorbutics and yellow fever patients.

Even as late as 1762, Monchy, a Dutch naval surgeon, who should have been familiar with scurvy and yellow fever as well, opines that "the malignant fevers" (of the West Indies) "are of the same nature with the scurvy," and quotes with approval the grouping by "the judicious Pringle" of "all these putrid diseases," scurvy, putrid and malignant fevers, and dysentery, into one class. Monchy also had "seen the simple remittents degenerate into the putrid yellow fever of the West Indies"—from an increase of intensity of the causal agent.

Spite of the fanciful nature of the above, one must always consider the possibility of scurvy in epidemics aboard ship. Nor can one differentiate it from yellow fever, also frequently a ship disease, by the difference in mortality. In long voyages, scurvy was sometimes a scourge, as in Anson's voyage around the world in 1740-44 (Bruce, 1759). In any detailed account, however, the epidemiologies of the two diseases should allow of differentiation. Scurvy would not begin until a considerable time had elapsed after leaving a port at which fresh provisions were taken aboard, or where the men had been allowed ashore enough to eat an abundance of fresh things. Yellow fever, or any other infection taken on at the port, should show shortly after leaving it. Scurvy would, and yellow fever might, increase indefinitely, both in severity and in the number of those sickening, as the time at sea lengthened, becoming ex-

tremely bad in very long voyages, while, given time enough, yellow fever would culminate, diminish, and, in a very long voyage, disappear. There would be no deaths in the early part of an epidemic of scurvy, while these would increase rapidly as it continued. The case mortality of yellow fever would, or might, be nearly as great at the beginning as at any other time. Men who develop scurvy and continue at sea under the same conditions do not get well but, if they get up, go down again and again, and become gradually worse. Those who recover from yellow fever suffer no second attack and get well; slowly and imperfectly, it may be, from improper food and hardship, but they do get well. Finally, from the very nature of scurvy, it rarely attacks the cabin or the galley until the forecastle is decimated. The writer has seen quite an amount of it, and has had to quiet one mutiny from men who believed that they were poisoned by their officers, else "Why should we be all sick and dying and all of them, and the cook too, well?"

### *Dengue*

Another insect-borne disease and, since it is conveyed by the *aegypti* mosquito (possibly not exclusively), naturally dengue occurs under much the same conditions as yellow fever. The parasite has, however, a much wider geographical distribution.

The first definitely recognizable account of dengue in America is that of Stedman (1828) at St. Thomas, 1827-28. It was reported as having been brought by a vessel from Africa, but "this is not ascertained." Whether it existed in America before may be a question.<sup>12</sup> Since that time it has quite certainly been frequently present in tropical America, but has been noted rarely except in the United States. It is extremely like mild yellow fever; so like, that, in the first stage, differentiation is generally impossible.

To judge by the records, dengue has been of wider dissemination and more frequent occurrence in the Levant—Eastern Asia and North Africa—where it probably originated. The writer thinks that its occurrence in tropical America has frequently not been recognized. When not epidemic, it has been grouped, by the Spanish, with the "acclimating fevers," and, by the French, with the "inflammatory fevers," with which, when yellow fever existed, were also grouped many mild cases of the latter disease. One epidemic at least, that of 1860 at Martinique, reported

<sup>12</sup> The account of the "*coup de barre*" at Guadeloupe in 1635, as given by du Tertre (1667-1671, vol. i, p. 81), fits dengue better than yellow fever, but was probably neither. This will be taken up in its place. (See Part iii, chapter xi.)



by Ballot (1870) as "*fièvre inflammatoire*," was unquestionably dengue. Rufz de Lavison (1869, p. 75) states that a previous epidemic of dengue there had been known as "*la giraffe*."<sup>13</sup>

Dengue is practically not a fatal disease, and *this* is our differential for epidemics of yellow fever and dengue. When Fernan del Busto y Blanco (quoted by Le Roy de Méricourt, 1867) gives an epidemic of yellow fever in Teneriffe in 1828 with over 5,000 cases and 60 deaths, dengue is the first thing of which one thinks. Although M'Kinlay (1852, p. 341) is quite sure that the "polka fever" of Brazil (1845-1848) was yellow fever, he questions whether this name "polka fever" ever would have entered into medical nosology had it not been a cherished tradition that yellow fever could not exist in Brazil. Yet when he adds that, except as a contributing cause of death in very weak people, it was practically without mortality, one questions his diagnosis.

Of its relation to yellow fever, he says, "this 'polka fever' had been yearly becoming more severe, until, in 1849, a fatal yellow fever was developed," i.e. from the "polka fever," or for which this had prepared the ground. This idea—the transformation of dengue fever into yellow fever, or its rôle as a forerunner of yellow fever—is common in the American literature of the last half of the nineteenth century (see Dutroulau's

<sup>13</sup> Dutroulau (1868, p. 131)—one of the princes in American or French tropical medicine—says of the "*fièvre inflammatoire*:" "*C'est la racine de la fièvre jaune dans ce climat*." He is speaking of that of the French Antilles. The claim that yellow fever existed continuously in the French Antilles as the "*fièvre inflammatoire*"—the so-called "larval form of yellow fever"—is, I think, without basis in fact. The fact that a company of young men, raised in Martinique, after a prolonged period of quiescence (of yellow fever) were decimated by yellow fever in Vera Cruz, where they had gone with Bazaine's expedition of 1862 (Corre, 1882, p. 63), seems sufficient evidence of this.

These islands freed themselves from the infection of yellow fever from time to time, as did others. As to the "*fièvre inflammatoire*," it included mild cases of yellow fever, when yellow fever was present; dengue, when dengue was present; some cases of "*embarras gastrique*"; and any other acute febrile manifestation—an "*omnium gatherum*" of acute febrile attacks not otherwise classified.

The writer saw dengue in Porto Rico (1915) mistaken for, and reported as, yellow fever. As "*el trancaso*," it was quite well known on the island, to the laity at all events, occurring from time to time as epidemics of greater or less extent in the towns, villages, and large plantations. This one was at San Juan, and the town was full of *aegypti*—that is, the soil prepared for yellow fever. This Porto Rican term "*trancaso*"—"a lick" (meaning a blow)—is identical with that, "*coup de barre*," used to designate the fever of Guadeloupe in 1635. Both express the sudden onset of the crippling pains which mark dengue and, quite generally, yellow fever as well.

statement in footnote on page 65). This is natural; the presence of dengue implies the prevalence of *aegypti*, and only the introduction of the microörganism of yellow fever is necessary for the occurrence of that disease. The "polka fever" was evidence of an infestation of the Brazilian coast towns with *aegypti* and, to that extent, a sign that the ground was prepared for the propagation of yellow fever, when introduced.

At times, doubtless, the two diseases coexist. If this is recognized, light cases of yellow fever will be reported as dengue, and, even if there be no dengue, a community being usually extremely loath to admit yellow fever, the light cases of yellow fever, and even the others, in the first stage of the disease, may be diagnosticated dengue, which will "turn into yellow fever" as deaths become common—a few deaths would be explained away.

### *Malarial fever*

Host-borne, by the *Anopheles* mosquito, prevalent in hot climates, and with some of its manifestations very like yellow fever, it would be a wonder if malaria were not confounded with yellow fever. Also, since malaria was well known in Europe, the tendency would be to consider yellow fever malaria rather than the reverse. This was done; and it was not until well into the nineteenth century that we find the two diseases generally recognized as distinct in nature, the older view being that yellow fever was merely an intense form of paludism.

William Fergusson (1823, pp. 294–295), Inspector General of Army Hospitals, groups as one the famous Walcheren fever in Zealand that crippled the Earl of Chatham's army, and that which he had seen on the tablelands of Barbados in the West Indies. He sums up: "the intensity" of the paludal fevers produced "from the common ague of the fens of Lincolnshire up to the aggravated yellow fever or malignant Remittent of the West Indies, may be measured almost to a certainty by the solar heat of the countries."

Chisholm (1801), on the other hand, made no mistake in his pathology when he pronounced the "Fever of Bulam" which he observed in 1793 as distinct from the fevers hitherto prevailing in Grenada and the adjacent Windward Islands. He did err in his nosology when he called the latter "the yellow fever of the West Indies." Yellow fever *had* existed in these islands, certainly in the latter part of the seventeenth century, from the so-called "epidemic of the *Oriflamme*"—the "*mal de Siam*." It had been confounded with the paludal fevers, and the severer cases of



these were called by its name. It had evidently disappeared entirely from these islands, and only the malarial fevers were left, to which, however, the name "yellow fever" was applied. So long had it been absent that, on this reintroduction from Africa, it was absolutely unknown to local physicians, and its ravages in the generally susceptible population, grown up since its last prevalence, fully justified Chisholm's quotation: (1801, vol. i, p. 121) "*nova pestis adest: cui nec virtute resisti, nec telis armisve potest*" (Ovid).

The point is that the malarial fevers of the Windward Islands—possibly only the severer cases of them—were then called "yellow fever" by both physicians and laity, even when no yellow fever was present, and when both malarial fever and yellow fever prevailed endemically they were considered identical, differing, if at all, only in intensity.

The opinion of Fergusson (1823) as to the identity of malarial and yellow fever, received the powerful official endorsement of the board which, in 1852, formulated the so famous, or infamous, "Second Report on Quarantine. Yellow Fever" (Gt. Britain: General Board of Health, 1852). This board was apparently called for the purpose of pronouncing that yellow fever was not communicable from man to man or from ship to shore, and simply declined to consider any evidence to the contrary. The report, indeed, is simply an *ex-parte* argument against the communicability of yellow fever anywhere and under any circumstances. This decision was reached despite the powerful opposition of Sir William Pym (1848), Inspector General of Naval Hospitals and Superintendent General of Quarantine, who knew more of the nature and epidemiology of yellow fever than any other man in Europe of his time, and against the definitely and logically sustained conclusions of the French Commissions, which had studied the great Spanish epidemics. Pym's arguments, with the array of admitted facts on which they were based, should have convinced St. Thomas himself, as should Audouard's (1822, 1824), but did not influence the decision of men who were determined to allow no maritime quarantine against yellow fever. From this time on, in British Africa especially, these two fevers were *officially* identical and were officially non-communicable, and, doubtless, a certain number of deaths were caused by this ruling—especially in vessels on the African stations.

While this was true of the leaders of the medical profession in Europe, few of whom, until the Spanish epidemic of 1820, had much chance to study yellow fever at first hand, a number of men in America, early brought into intimate relation with yellow fever, saw in it, as did Chisholm and Pym, a disease different in kind from malarial fever.

The early Spanish accounts ascribe all their epidemics to the same conditions of excessive heat and moisture as they associate with paludism, adding privation, hardship, and the sinfulness of the colonists as being contributory. As buboes were always included among the manifestations of "*la peste*," the latter was doubtless, to an extent, causal.<sup>14</sup>

The same is true of the causes of the "*coup de barre*" as given by Père du Tertre (1667–71) and by Breton (1665) (whom the former copies), yet du Tertre speaks years later of intermittent fevers, tertian and quotidian, as existing in Guadeloupe; and Breton, (1665, p. 464) in his Dictionary, gives a Carib word—"tíoüaonochéarou" as equivalent to "*la fièvre est intermittante*." These French ecclesiastics, then, certainly from 1648, when "*la peste*" appeared in Guadeloupe, recognized yellow fever as distinct from paludism, and this view continued in the French Antilles. So in Barbados, Gamble (quoted by Hughes, 1750, *re* epidemic of 1691), Towne (1726), Warren (1740)—who thought it was the Levantine plague—Hughes (1750), and Hillary (1759), ordinary practitioners or laymen, all recognized their epidemics as different from paludic fevers, as, I judge, did many naval surgeons whose vessels were infected at Bridgetown. This was natural at Barbados, there being, I presume, no malaria on the island then, as there is none now. Indeed, the absence of paludic fevers of local origin is asserted by several of the writers above quoted.

In most places in which yellow fever occurred in America, however, malaria was prevalent, and in many of them, severe. It happened, then, that the diseases were not differentiated, except in sharp epidemics, and not always then. It is true that, taught by the Philadelphia epidemics, the celebrated La Roche (1855) regarded the diseases as distinct;<sup>15</sup> yet we find Nott in Mobile (1847) and Faget, of New Orleans, as late as 1875, still under the necessity of *arguing* for the *specificity* of yellow fever.

<sup>14</sup> Oviedo is the only one of the early chroniclers who recognized syphilis as a specific entity, and understood how it was contracted. He was a soldier; and the others, except Bernal Diaz, were ecclesiastics.

<sup>15</sup> La Roche believed in the spontaneous origin of both yellow fever and malaria from heat and the decomposition of organic matters—the first mainly from animal, the second from vegetable sources—and that neither was communicable from man to man. His contemporary and rival, Benjamin Rush (1796, 1799, 1803, and 1805) believed them the same disease (indeed he held the doctrine of the "unity of disease") in their spontaneous origin, as above, and, until his later years, believed in the communicability ("contagion") of both. Later, he became a non-contagionist and was exceedingly vehement in this view, as he had been in the opposite one.



The former evidently considers himself as being distinctly on the minority side when he argues against its being caused by a gaseous miasm—"the commonly accepted cause of the paludal fevers."

The writer, however, is inclined to think that malaria did not exist in America prior to its discovery. It would of necessity have been introduced early by the Europeans and negroes, and, *Anopheles* being generally present, it would have spread. Still, this spread was not immediate or universal, and the absence of malaria in some places must have made the strange disease—"peste," "contágio," "fièvre pestilentielle," or even "*la enfermedad*"—more easily recognized as peculiar to the country.

On the other hand, about Senegal and the Gulf of Guinea, the regions in which we first suspect and first definitely find yellow fever in Africa, malaria was very prevalent and very severe when the country was first visited by Europeans. Indeed, it is generally held that malaria was indigenous to Africa, though not necessarily to this part of it. Moreover, until about the middle of the eighteenth century, Europeans, who alone would show it, were not numerous enough at any place on this coast to make an epidemic of yellow fever distinguishable from those due to malarial fevers. Differentiation of these diseases, then, would be made less readily—and hence later—in Africa than in America; and in point of fact, the differentiation in Africa was not accepted until after, and doubtless on account of, their recognition as different diseases in America.

The first recognition of yellow fever in Africa, as distinct from malarial fever, was by Schotte (1782) in the severe epidemic among British troops at St. Louis de Senegal in 1778.<sup>16</sup> He called it "*Synochus atrabiliosa*" and differentiated it clinically from the malarial fevers that he knew, but seems not to have considered it different from them in origin. Moreau de Jonnès (1822), from the histories given, recognized "the yellow fever of the Antilles" as occurring in the ill-fated expedition of Captain Tuckey to discover the source of the Zaire (Congo) in 1816, and discussed the question whether it was produced spontaneously where found or brought from America by slavers. He recognized it as frankly communicable.

William Fergusson (1839) made the same differentiation from his

<sup>16</sup> Bérenger-Féraud (1890, p. 27) is of the opinion that there may have been yellow fever aboard ships in the Bight of Benin as early as 1520, and in several expeditions into this region, from 1553 to 1588. Some of these outbreaks were quite certainly malarial fever, but this will be discussed later. (See Part III, Chapter XV.)

own observations on the west coast of Africa, and is as definite as to the communicability of yellow fever as is Moreau de Jonnès or Pym. Nevertheless, that malarial fever and yellow fever were identical, differing only in degree, and not communicable in any way, directly or indirectly, from the well to the sick, was made as the official decision in 1852 and was quite generally accepted in British West Africa for some time later.

Pym (1848) used the terms "Fever of Bulam," "*Vomito Negro* Fever," reserving the name "yellow fever" for *malaria* of a severe type, as did Chisholm (1801). Ferguson, in 1839, used the term "yellow fever" in the same sense we do now.

Even after the two diseases were differentiated, the tendency, wherever yellow fever was known, was to consider all severe, general, and fatal epidemics as "yellow fever" and the less severe ones paludism. In the United States, isolated severe cases, whether of malaria or of yellow fever, were usually reported as "sporadic cases" of "yellow fever," and interpreted as showing a tendency of the "atmosphere," or sometimes more conservatively "of diseases," "to take on a malignant aspect," which, if the cases happened to be yellow fever, and occurred in the early part of the summer, was not infrequently verified. This was not common in Europe. The great epidemic of malaria in the Earl of Chatham's army at Walcheren, in Zealand, was, indeed, called "yellow fever" by Fergusson (1823) and, in the famous Second Report of Quarantine (Gt. Britain: Gen'l Board of Health, 1852). Also, Sir William Burnett (1814, p. 257) gave the same name to the malarial fevers of the Mediterranean Islands. Besides these, I know of no others.<sup>17</sup>

In some regions, however, and notably in the United States, quite a number of outbreaks of severe malaria, especially of blackwater fever, have been reported as yellow fever. That mentioned by Daniel Drake (1850, p. 134), at Memphis, in 1828, must have been one of these, as yellow fever failed to propagate itself here, even in 1853. The writer has personally investigated a number of such. He has, however, rarely seen yellow fever called malaria in the United States. It is more apt to masquerade as dengue. In Africa, on the contrary, the tendency has

<sup>17</sup> The small epidemics reported as yellow fever by Baron Larrey (1812-17, vol. ii, pp. 24-26), on the authority of Dr. Witman's Journal, among British troops near Jaffa, in Syria, were quite clearly malaria. That reported tentatively by Larrey (1803, p. 104), Desgenettes (1802), and Pugnet (1804) among the French around St. Jean d'Acre and at Damietta about the same time (1798-1799) was probably, some of it, plague, some malaria, and some of it hospital gangrene.



been in the British colonies, until very recently, to report yellow fever as malarial fever. Of this, I think, there can be no doubt.

## DIFFERENTIATION BETWEEN YELLOW FEVER AND MALARIA

### *I. In epidemiology*

A. *General.* (1) Yellow fever is essentially a disease of towns and cities; malaria as essentially rural, or at most suburban. For instance, it seems scarcely reasonable to consider the "*modorra*," so destructive to the Guanches of Teneriffe in 1494, as yellow fever. The Guanches were distinctly a rural people, while the Spaniards, living together in a fortified camp and seaport, were not attacked.

(2) Yellow fever is contracted aboard ship, off-shore and on the high seas, and is conveyed from ship to ship and from ship to shore. Malaria is not contracted aboard ship, unless the vessel is lying, or has very recently lain, close inshore. It may, however, be contracted elsewhere and develop aboard ship, even some time after leaving land, under conditions which may lead one to mistake its source. It is not conveyed as an infection from ship to ship off-shore, because active *Anopheles* mosquitoes, unless confined in some close compartment, as the hold, would not remain aboard ship more than 36 to 48, possibly 60, hours.<sup>18</sup>

(3) Yellow fever is contracted in some places on land known to be free from malaria—as Barbados, Ascension Island, Païta, etc.

(4) The duration of an epidemic, or even the infection, of yellow fever at any place is limited *unless new human material be supplied*, and this independently of climatic conditions. Malaria lasts, or recurs, year after year, unless the conditions of *Anopheles* production, or protection from them, are changed. Yellow fever at Cadiz, severe the first year, lasted only two years, 1730 and 1731, (Villalba, 1803, vol. ii), while the epidemics of tertian, at and about Cartagena, are noted from

<sup>18</sup> The writer has seen vessels—and many vessels—with malarial fever contracted at Colon, come into the Gulf Quarantine Station: "No one on the sick list until — days out," "when a cold wind coming up," or "all hands wet in a cold squall, — men went down with fever." In one case (the German bark *Jacobine*) the days out were 10 and the number of men sickening within a few days were 15—all there were aboard—with 4 deaths before arrival. Two of the dead men (autopsied) and the ill living ones certainly had malarial fever. In the American bark *Zingara*, from Lagos, no one was sick until over three weeks out, yet all came into the Marine Hospital at Chelsea (Boston) with malaria, except the negro cook and the master. The latter went home sick and, my recollection is, died.

1764 to 1771, and again from 1776 to 1779 and from 1781 to 1787. There are hundreds of such instances; and this is a reliable index of which disease prevails *if they prevail separately*.

B. *Incidence and mortality*. (1) Old residents show immunity to yellow fever in places where it has been of frequent and recent occurrence.

(2) Recurrent attacks, common in malaria, are of extreme rarity in yellow fever.

(3) The death rate among those attacked is high in yellow fever. Malaria is usually far less fatal, to whites at any rate, yet some forms of it are fatal enough. It would be more correct to say that its *case mortality* is much less than that of yellow fever. The *community mortality* of men continuously exposed to severe malaria, from *repeated attacks*, may show a high ratio to the population. This was especially true before quinine was in general use. The element of time is important here—a high community death rate in a *short* time would point to yellow fever; in a longer period it need not.<sup>19</sup>

(4) Death rates from yellow fever among negroes are markedly lower than among whites, or, so far as we know, any other race. Malaria also is less severe and its death rates are less in negroes than in whites, but the difference is very much greater in the case of yellow fever than with malaria. In pure-blooded negroes, yellow fever is of low mortality, almost a non-fatal disease. An epidemic, then, with a high mortality among negroes is little apt to be yellow fever.<sup>20</sup> If both races are involved, the white *case fatality* should be higher for both yellow fever and malaria, but the *ratio of difference* should be markedly higher for yellow

<sup>19</sup> How great a death rate malaria can cause in a non-European population long free from it may be seen from the epidemic in Mauritius in 1867 and 1868. In Port Louis, out of 80,000 people, 22,231 died in 13 months, 234 in a single day, 31,920 on the island (Balfour, 1920-21). This, except for the length of the epidemic, reads like yellow fever. A large town was severely stricken, and the use of quinine was well known at that time. Moreover, we are told (Fowler, 1908) that the variety of soil "*which is most common*," (italics mine) "is porous and allows water to drain rapidly through it. It contains few springs. This is a healthy" (not malarious) "soil." Balfour "firmly believes" that "if Mauritius had not possessed in many parts so porous a soil and been blessed with such a remarkable winter climate"—warm weather is not continuous here—"it might easily have been ruined, as was ancient Greece, and as were large tracts of ancient Italy and Spain." One cannot but agree with him.

<sup>20</sup> An epidemic with a decidedly *higher* mortality among blacks than whites is apt to be smallpox, influenza, pneumonia, plague, or certain spirochetal infections—as recently occurred in Senegal (Noc and Nogue, 1921). Whooping cough, too, is very fatal among negroes under five years of age.



fever. This might not always be true if the negroes, from long residence in a malarious locality, had acquired a great deal of immunity to the local disease, and the whites were newcomers. A smaller case incidence would also have been *reported* among the blacks. Probably the case incidence is smaller, but certainly light cases are not recognized among them.

(5) In yellow fever the death rate among children is lower than among adults. The reverse is true of malaria, and the difference for both is greater in young children. The difference here is, however, less than that between the black and white races.

## II. In symptomatology

Features characteristic of yellow fever and likely to have been noted by the older historians, are:

(1) Sudden attack of a man in full health, with severe pains.

(2) Short duration of attack—to either death or recovery. Death usually comes quicker with yellow fever than with malaria, the great majority of yellow fever deaths being on the sixth and seventh days and many on the fifth. Yet the deaths that occur *within 12 to 24 hours* of the seizure are nearly all due to malaria. I say “nearly all,” for fulminating cases of yellow fever—“*el accésio pernicioso*”—do occur; a number in some epidemics. Far more commonly “walking cases,” sick for some days and dying suddenly, are reported as fulminating cases. Both together, in my observation, are rare compared with the deaths due to the fulminant forms of malaria.<sup>21</sup>

(3) Death frequently in convulsions—and often in coma.

(4) Persistent vomiting, black vomit, hemorrhages from about the mouth, and yellowness of the eyes and skin during the attack and in convalescence.<sup>22</sup>

<sup>21</sup> Good old Père Labat (1722, vol. i, p. 73), with the optimism of one immune by attack—and doubtless partly to make up for the rather horrific picture he had drawn of it—speaks this good word for “*le mal de Siam*”: “*Ce que cette maladie avoit de commode, c’est qu’elle emportoit les gens en fort peu de temps, six ou sept jours tout au plus terminoient l’affaire.*”

<sup>22</sup> The effect of treatment at times must have profoundly influenced the symptomatology of both yellow fever and malaria, and their death rates as well. Where antimony was used to any extent, vomiting was to be expected; and sufficient bleeding might produce temporary anemia in even a patient convalescent from yellow fever—that is, if he convalesced! It is fair to say, however, that tropical practitioners were very generally opposed to both of these agents as being, in their experience, injurious in fevers, spite of the “teachings of the schools” of the time.

(5) Extreme weakness in early convalescence is characteristic of yellow fever, but convalescence is rapid and complete, and not marked by anemia. In malaria, convalescence is slow and imperfect, may be interrupted by recurrent attacks of the original infection, and one is apt to be weak and anemic for a long time. Anemia is in no wise a sequel of yellow fever. Convalescents from yellow fever are frequently very yellow at first, but this does not last. The pasty, puffed face with, it may be, prolonged jaundice, follows malaria, not yellow fever. The Spaniards returning to Spain from Santo Domingo, whom Oviedo noted as "of such a bad color," "citroned and of the color of saffron or treacle," were quite certainly not in this condition as a result of yellow fever, for it was a voyage of some months from Hispaniola to Spain.<sup>23</sup>

Since this passage from Oviedo is so frequently quoted (by Moreau de Jonnès, Cornilliac, Finlay, and others) as evidence that "*la enfermedad*" at Santo Domingo was yellow fever, it seems fair to give it in its entirety:

"Thus, in order to live, from no beast or animal of the number I have mentioned, did they abstain, because as many as they could obtain went to the fire and, boiled or roasted, there was not lacking to their necessity appetite to eat these things so inimical to health and so terrifying to sight." He is referring to dogs, wood rats, lizards, jutias, iguanas ("a serpent with four feet"), and "serpents many-colored, but not poisonous," etc. "From the which, and from the humidity and the fatness of the land, followed many illnesses grave and incurable to those who were left with life. And from this cause those first Spaniards who came here, when they returned to Spain, some of those who came in this search for gold, if they returned there, it was with the same color of it, but not with its lustre, but citroned and of the color of saffron or treacle, and so infirm that immediately, or shortly after they returned there, they died on account of what they had suffered here and because the provisions and the bread of Spain are of stronger digestion" (I think he means harder to digest) "than these herbs and bad viands they eat here, and the airs more delicate and cold than those of this land. So that, although they returned to Castile, soon they gave an end to their lives, having arrived there" (Oviedo, 1851-55, *t. i, lib. 2, p. 50*).

To the writer, this speaks strongly for malaria, not yellow fever.

In general, then, an epidemic of malaria can be distinguished from one of yellow fever. The first is especially marked by debility; the second by deaths. As we have said, however, if the exposure to malaria be

<sup>23</sup> The vessels on which these "*azafranos*" returned to Spain left Isabela March 10 and arrived in Spain June 11 (Irving, 1892, vol. ii, p. 75).



continued, there may be many deaths. The experience of Shafter's army at Santiago in 1898 illustrates the first; that of the British under Albermarle in 1762, the second. The rather exceptional Mauritius epidemic of 1867 (Balfour, 1920-21) demonstrates the effect of continuous exposure to severe malaria. In the first case there was only a moderate death rate, but an almost complete destruction of the army as a fighting machine. Albermarle lost half of his force by death, but captured Havana. Had Shafter remained in Cuba, his mortality would have been increased and in time might possibly have approached that of Mauritius.

#### SICKNESS AND MORTALITY AMONG EARLY COLONISTS IN NEW COUNTRIES

There seems to be an unconscious bias among writers on the early history of yellow fever to ascribe to that disease all undue mortality among the newcomers to places in the tropics. Yet we know that high mortality was usual among colonists, and in expeditions to new countries under conditions where yellow fever could not possibly be accused.

The Jamestown colony, founded May 13, 1607, seems to be especially a case in point. Of the 105 men left at Jamestown by Newport when he sailed for England, June 22, "more than fifty were in their graves" by the end of September; and on his return to the colony, in January, 1608, he found only 38 survivors, little over one third (Fiske, 1900, vol. i, pp. 94-99).

There had been no trouble with Indians (Fiske mentions only two men killed by them), by whom some food was supplied. It was noted that the colonists had "crabs and sturgeon" from the river, and they had brought chickens with them, which should have increased. Also, being a small number, yet large enough to defend themselves, it had been easier to live off a wild country than if they were a larger body. They were fortunate, too, in their Governor, John Smith. He was an unusually able and energetic man, forceful, provident, and wise. He impressed, and had much influence with, the Indians, but he neither oppressed nor offended them, nor suffered any of his company to do so.

Yet of hardships, unaccustomed mode of life, starvation, and "fevers," a half died in three and a half months. Indeed, the last named factor, if they were malarial fevers, could not have been active more than two months—or at most two and a half—as paludal fevers in that region rarely show to any extent until well in August.

Nor was it only in the beginning of the colony that this high mortality prevailed. Many things witness to an excessive mortality among the

newcomers, and for a long time. As late as 1671 the then governor, Sir William Berkeley, said that of the indentured servants, "four out of five died during their seasoning period" (Tyler, 1900, p. 66), and these were not "*pauvres gens ramassez*," as were the first settlers of St. Christophe, but, as Tyler says, "self-respecting honest people, who came at their own expense or voluntarily entered into a term of service." In the same period, in the time of Bacon's Rebellion (1674), in the testimony to show that Bacon proposed "to resist against even the King his Majesty," Colonel Goode quotes him as saying that even if soldiers come from England "the Country or Clime not agreeing with their Constitutions," they being newcomers, "great mortality will happen amongst them in their Seasoning, which will wear and weary them out." And with this statement of fact, however much he disapproved of the disloyalty inspiring it, Goode seemed to agree (Fiske, 1900, vol. ii, p. 72). There are many records to show that this belief in the greater liability to disease and death of newcomers as compared with the "seasoned" men was general in Virginia for a long time, it may be for seventy-five years or more, after the colony was established, and that it was based on fact.

Such conditions—at least in the first years of settlement—were not peculiar to Jamestown. One hundred and twenty men landed at the mouth of the Kennebec, August, 1607. So many died that those left abandoned the colony. Of the 100 who sailed in the *Mayflower*, 51 died the first winter at Plymouth (Fiske, 1899, p. 82). At one time only seven men, including Elder Brewster and Miles Standish, were able to be about. Lane's colony, in the year spent at Pamlico, before they were taken off by Sir Francis Drake, lost half their number. It was worse with Laudonnière's colony in Florida, which, as Fiske says (1900, vol. i, p. 20), suffered "the frightful hardships that were the lot of most newcomers to the American wilderness," with the consequent mortality. Almost every colony established by Europeans, *brought directly from Europe* to North America, suffered excessive sickness and mortality. Manhattan and Philadelphia are among the exceptions. The mortality among the early settlers in Australia, in modern times, was nearly as high as those just given.

Independently of yellow fever, the conditions in the European colonies in the wet tropics should have been less healthful than at Jamestown, both from climate and environment; and excessive mortality in them is not, of itself, presumptive evidence of yellow fever, even if it is much greater among the newcomers than among the "seasoned" men.

Much depends on the preparation for settlement, and more on the



men sent out. Guadeloupe and Martinique were colonized by the French in 1635, in June and July respectively. The colonists for the former, numbering about 550, came direct from France—“*pauvres engagez*” raised in Dieppe—and arrived with insufficient provisions to sustain them until they could establish themselves. After two months, the supplies brought from France being consumed, a famine—characterized as “horrible”—began. This, says du Tertre (vol. i, pp. 78–81), was followed by a heavy mortality due (1) to the famine; (2) to sickness—the “*coup de barre*,” and (3) to the cruelty of the overseers of the work, who forced the colonists “by blows and by severity to work in clearing the woods and in all weathers.” To this last, indeed, he ascribes the greater part of the mortality—“because the French were much enfeebled and were treated worse than slaves in Barbary.”

Martinique was settled by d’Enambuc, who had been long in the West Indies, with 100 men—note the small number, who could therefore live off the country—“old inhabitants of the island of St. Christophe,” “*tous gens de main*,” “accustomed to the climate, to the work and the fatigues of the country.” They were “very skilful in clearing land, planting and cultivating provision, and very adroit in building houses.” They brought with them, “as provision and to plant, all things necessary,” among which are mentioned manioc, yams, peas, beans, and other grains—probably Indian corn. It is expressly stated that the Guadeloupe colonists made no provision for future crops, being obliged later to send to St. Christophe for seed to plant, and in the meantime depending entirely on supplies from France that never came (du Tertre, 1667, vol. i).

No special mortality was reported at Martinique until the “*mal de Siam*” of 1690, and it may be claimed that the men from St. Kitts, some of whom were quite certainly buccaneers, were immune to yellow fever. Yet the fact that both they and d’Enambuc were used to tropical conditions and were “men of their hands” cannot be left out of consideration in accounting for this.

It is common enough, then, to have much sickness and many deaths in a newly planted colony, due simply to ignorance, lack of good administration, and personnel unsuited to pioneer conditions. This should not continue, however, without some pathogenic reason. When, after some years, Plymouth had been well organized, the disproportionate mortality ceased. This lasted longer with Virginia, partly at least on account of the prevalence of malaria; yet, as the colony became self-supporting and prosperous, it disappeared, although “*agues, epidemics*”

and *fluxes*" are reported each fall. Tide-water Virginia abounding in *Anopheles* mosquitoes, and the life being essentially rural, there was an excessive mortality among newcomers for a considerable time. Even in Guadeloupe, after the famine, when things had settled down, one hears no more of excessive sickness until the advent of "*la peste*" in 1648; of none in Martinique until the "*mal de Siam*" in 1690, although it was visited by, and suffered from, the "*peste*" in 1648 also.<sup>24</sup>

When, then, in an established colony, with a fairly settled life, one finds much sickness and many deaths *among newcomers only*, the conclusion is justifiable that this is due to some disease producing immunity against its own attacks. In the American and West African tropics that would most likely be yellow fever, although a certain immunity, far less complete than against yellow fever, is produced by malaria, almost everywhere present.

If there be also regularly an excessive mortality among the young children of the colonists, malaria is more probably the cause than yellow fever, especially if this occurs under rural or suburban conditions of life. Yellow fever, indeed, does raise the death rate of children, but not with the steady persistence of malaria.

The sickness and heavy death rate among newcomers—and the latter confined to them—during much of the eighteenth and early part of the nineteenth centuries in Jamaica, Barbados, and the French Antilles, the same conditions existing to the beginning of the twentieth in Havana, Rio de Janeiro, and Vera Cruz is, even by itself, very strong evidence of the presence of yellow fever in those places during the times stated. One must lay stress on the "much sickness" and the "many deaths," because the newcomers do not know how to take care of themselves, and do not take care of themselves, in the tropics as do old residents. There is an acclimatization to environment and to climate when the changes in them are great—which lessens one's susceptibility to disease—as well as to disease by previous attack. This is seen even in new troops, as compared with old ones, when there is no change of climate, only of conditions of life.

<sup>24</sup> Père Bouton, writing in 1640 of Martinique, which he left that year, does not mention the "*coup de barre*" or any dangerous disease on the island, although he gives a rather full account of those that occur: yaws, ulcers, and malaria. The language used seems to be intended for both islands, Martinique and Guadeloupe: "It is marvelous if each one who arrives *in these islands*" (*italics mine*) "has not four or five attacks of fever," which he regards as of small importance, advising to continue about and not pay too much attention to them.



PART III

THE PLACE OF ORIGIN OF YELLOW FEVER





## CHAPTER I

### GENERAL CONSIDERATIONS

In two regions only has yellow fever been known to have been permanently endemic, tropical America and West Africa. It does not seem necessary, then, to consider any others as a possible place of origin: they are either (a) those in which the disease has not appeared at all or (b) those in which it failed to establish itself permanently when it did appear in them. Of the latter, it is to be noted that we have for the western Mediterranean countries a very long and reliable history, medical and general, with nothing in it to show the existence of yellow fever in these countries in ancient times or until well after communication was established with both America and West Africa.<sup>1</sup> The same is true, only the history is longer, of the countries of the eastern Mediterranean. Certainly the "*orbis terrae*" of the Ancients—"the world and they that dwell therein" of the Psalter—may be exonerated. The claims of, or rather the charge against, the other two regions require examination.

There are data of two kinds on this problem: biological and historical. Let us take them in this order:

#### BIOLOGICAL EVIDENCE ON PLACE OF ORIGIN OF YELLOW FEVER

1. The reaction of the negro to yellow fever is, I think, just what one would expect to be evolved in a race for many generations subject to that infection. Negroes contract the disease, so far as we know, as readily as other races, but have it more mildly and rarely die of it. Thus, they apparently are as susceptible to the *infection* as other races, but less susceptible to the *toxins*; and this is what we would expect in the evolution of a race long subject to yellow fever. The American Indian, on the contrary, has shown no immunity either to the infection or to the toxin. He contracts yellow fever and dies of it as readily as a white man.

2. Of its known vector, *Aedes (Stegomyia) aegypti*, there is this to say:

<sup>1</sup> The first generally accepted epidemic of yellow fever in Europe, and I think the first, was the "*pestilencia*" of Cadiz in 1730, when 2,200 deaths occurred in September and October. This showed after the arrival of the flotilla of Pintado from Cartagena de las Indias (Villalba, 1803, vol. 2, p. 112-113), "where it had lost many of its men with '*vómito prieto*'" (Ulloa, 1748, vol. 1, p. 61). At this time this disease was an old story in America.

(a) This insect, as we have shown (page 24), is distributed by vessels, especially by the old sailing vessels, breeding aboard them en route; and no matter in what seaport it first existed in permanence it would ultimately be distributed from it to all others (climatic conditions allowing) to which vessels from the first port went. Ultimately, when the interlocking commerce in vessels between the ports of the world had gone on long enough, this mosquito would be found in all ports in which we had the conditions of temperature and breeding places necessary for its continuous propagation and existence. Its presence now in any place is, then, no evidence that it originated there.

(b) It would seem that this species, so definitely differentiated in its biology as a commensal of man, and not regarded by entomologists as one of the older forms, developed from some similar one with less sharply marked characteristics, and that, in the region where this took place, one would be apt to find a number of forms allied to it, but less completely differentiated; that is, different species of the same subgenus and with similar, but not quite the same, life history.

Now no other species of this subgenus (*Stegomyia*) is listed as occurring in the Americas. And for this one, its occurrence in America as early as we believe we definitely recognize yellow fever there (1648), about 150 years after intimate communication between West Africa and tropical America by slavers, would, for the reason given above, mean little or nothing for its American origin, especially as we know that the area of its infestation in the New World has been considerably increased in recent historic times.

On the other hand, there are many species of *Stegomyia* in West Africa and other regions of the Old World, some of them breeding with *aegypti*, and of fairly similar biology. It seems, then, to be essentially an Old World form, that is, to have been evolved there. That this species (*aegypti*) is of Old World origin is, I think, held by most of the leading entomologists: by Howard, Chief of the U. S. Bureau of Entomology; by Dyar, and by the late Frederick Knab of the same bureau; by Edwards, Entomologist of the British Museum (stated by the first two in verbal communication, and by the last by letter (March 23, 1923))<sup>2</sup>; and,

<sup>2</sup> I quote from a letter of Edwards, March 23, 1923: "I entirely agree with your suggestion that the fact that *S. fasciata* (*argenteus*: *aegypti*) has no near American relatives, but a number in Africa, argues rather strongly for the West African origin of the disease, even though there might be some possibility of its having been introduced into the Americas in some prehistoric period, which may not be likely. It is indeed certain that there are several species of this subgenus in



although from other reasons than those given above, is strongly advocated by Göldi of Pará (1905, p. 104).

If this species originated in the Old World it does not seem reasonable that it should have reached America prior to the discovery of 1492, accompanying an early immigration of men from the Old World, since this immigration was most probably by land and, for a long time, through what must have been, even then, cold regions. If this insect reached America only after 1492, yellow fever could not have existed in America prior to that date.

Such biological data, then, as are given by both human and insect host are entirely in favor of an Old World, that is, West African, origin of yellow fever. Since, however, we can only say that the reaction of the negro race to yellow fever is what one would expect to be produced in a race long subject to that infection, not that it was positively produced by that cause; and that the facts known of the vector render it extremely probable that this species had its origin in the Old World, not that it positively did originate there, the most the above biological data and argument can do is to produce what logicians call an "antecedent probability" of this, to the writer, of decided weight; but of its weight each one must judge for himself.

#### HISTORICAL EVIDENCE ON PLACE OF ORIGIN OF YELLOW FEVER

Yellow fever was recognized as a disease *sui generis* in America long before it was so recognized in Africa; and before this differentiation from other diseases we can quite certainly, I think, from the old descriptions of indefinitely and variously named epidemics, recognize it as being in America before we can claim this for Africa.<sup>3</sup>

---

Africa which are very closely allied to the yellow fever vector, both on adult and larval morphology, and in their habits, although *S. fasciata* is the only one which has developed a special association with man. These allied species all breed in tree-holes, leaf axils, or other such places where there are small natural collections of water, and it is perhaps of some significance that *S. fasciata* still shows a distinct preference for such places so long as they are obtainable within the near neighborhood of human settlements.

"There is also another matter which seems to me to point in the same direction, that is the distribution of *S. fasciata* in Africa. On that continent it is extremely widespread, being found far into the interior in almost every village. I believe I am correct in saying that in America, and also in the Oriental and Australasian regions, it is more abundant in seaports, outside which it has a very limited distribution."

<sup>3</sup> The first account of a sickness that can definitely and with certainty be recognized in the New World as yellow fever is, I think, that of 1648 in Guadeloupe and

Nevertheless, these statements are less convincing as to the American origin of yellow fever than one at first sight might think. These two regions were brought into communication with Europeans practically at the same time, and both before—and long before—yellow fever was recognized in either by Europeans. And, with two exceptions to be noted later, we have only European history to guide us in this matter. Cape Verde was rounded in 1444; the Guinea Coast, the region under suspicion, was visited in 1471, and the first settlement, *São Jorge da Mina* (now Elmina), was made in 1482, only 11 years before Columbus founded Isabela.

Moreover, the communication between tropical America and this newly discovered part of Africa began early and was intimate, so that not only yellow fever, but its insect vector, might well have been transferred from either one to the other many times and have been well established in its new home before the disease was in fact recognized in either country. The first general license for the slave trade to the Spanish Indies was not issued (by Charles V) until 1516; but Ovando, the Governor of Hispaniola, in 1503 petitions that no more negroes be allowed to come in as they were already too many for good order (Herrera *v. i, dec. 1, lib. 5*). It is this trade which, almost from the discovery of America, formed an intimate bond between West Africa and Spanish and Portuguese—and later French and English—America. This quite certainly carried yellow fever backwards and forwards between them until each one accused the other, and with truth, of being the immediate source of its infection. For example, the “fever of Bulam” was brought from the West African (Guinea) Coast in 1793 to the Island of Grenada, whence it spread widely over the Little Antilles and the South Caribbean littoral, while in 1866 the *Rosa del Turia*, leaving Havana, with nearly 200 *déportés*, carried infection to Fernando Po, where it made great ravages and spread to other islands in the Gulf of Guinea (Yglesias y Pardo, quoted by H. Rey, 1878). One might instance a number of other cross-

---

other French Antilles (du Tertre, 1667–71, vol. i, p. 422) and in Yucatan (Lopez de Cogolludo, 1688).

In Africa, although Lind’s (1792) account of fever aboard vessels off the coast of Senegal in 1768 is usually accepted as the first in which we can definitely recognize yellow fever for that continent, yet we must await Schotte’s report (1782) of the epidemic of 1778 among the British troops at St. Louis de Senegal to find anything approaching the certainty of the yellow fever of 1648 described by Cogolludo in Yucatan. The earlier, but less definite, data from the Slave Coast will be taken up later.



transferences, as when the frigate *Nivaria* (1862) infected the Canaries (Méliér, 1863, p. 194) etc., etc. There are several such instances recorded, and doubtless others that escaped record; and even in very recent times Flu (1910) states that the yellow fever which was epidemic in Togoland when he wrote was "imported from America, probably before 1896."<sup>4</sup>

#### REGIONS FOR EXAMINATION IN AMERICA

Knowing the conditions necessary for the continual existence of yellow fever, there were but few regions in the Americas, prior to their discovery by Columbus, in which we would expect this to occur, that is, which would serve as permanent endemic foci, from which, however, the disease would, or might, from time to time, spread as epidemics elsewhere. These regions are:

1. The Gulf Coast of Mexico, the "*tierra caliente*" of that country.
2. The Maya country of Yucatan and Central America.
3. The coastal region of Peru, especially that of the Chimu civilization, centering around the present city of Trujillo.
4. The scattered area about the Caribbean, mainland and islands, linked together as a unit by the voyages of the Caribs and, to an extent, occupied by them.

There are no other regions save the first three at once warm enough for the continuous activity of *aegypti* and in which there was sufficient concentration of population to furnish the supply of susceptible men necessary to continue the infection indefinitely. There was nowhere any great concentration of population among the Caribs, but the movement of these seafarers from place to place would, or might, have the same effect in continuing infection as would the existence of a much larger more sedentary population.

If yellow fever were originally permanent in either one of the first two regions, the communication between them would probably be sufficient, in course of time, to carry it to the other, where it would prevail, in epidemic form at first, to become permanently endemic, or not, according to the sociological conditions that existed there. If originally only *long continuing*—as compared with *permanent*—in any one, it might be carried from time to time to others and *vice versa*, so that they together might

<sup>4</sup> No reference is given for this statement, and the writer has no knowledge of its basis. Flu, however, is an extremely careful and reliable man and a trained epidemiologist, and the writer is inclined to accept his statement as he gives it.



FIG. 2. TROPICAL AM





form a permanent regional focus, although this might be true of neither one separately.

The Peruvian area may be eliminated at once. It was absolutely isolated from any other concentration of population in regions warm enough for continuous *aegypti* activity, and in this region, the history of which is quite well known, no epidemic of any kind is recorded either among the natives or the Spaniards until well after the Conquest, when European diseases—smallpox and typhus—wrought havoc among the natives.

Of the Carib region there is this to say: that in this area, after settlement of Europeans, when susceptible immigration, population, and intercommunication were all greater than in pre-Columbian days, yellow fever did not maintain itself continuously, but died out more than once.<sup>5</sup> It seems unreasonable, then, to think that this Carib area could have remained, by itself, a permanent focus. However, by occasional communications with the other regional foci just mentioned, it might have been the seat of scattered, and hence long continued, epidemics, and thus have assisted in maintaining that focus, forming a part of it indeed.<sup>6</sup> There seems, however, to have been very little, if any, contact between these seafarers and either the Mexicans or Mayas.<sup>7</sup>

<sup>5</sup> The “*peste*” of 1648 in the Little Antilles raged as an *epidemic*, and hence was a new introduction. The same is true of the “*mal de Siam*” in 1690 and of the Bulam Fever of 1793.

<sup>6</sup> Even Dr. Carlos Finlay (*nomen clarum et venerabile!*), who was a most earnest advocate of the American origin of yellow fever, is inclined to confine its permanent endemicity, before 1495, to the mainland. Among his “conclusions” are (Finlay, 1912, p. 125):

“(1) Before the discovery of America by the Spaniards yellow fever was endemic in the coasts of the Sea of the North of New Spain” (Mexico) “(Vera Cruz in particular) and in the *Tierra-firme* (Darien: Nombre de Dios) . . . .

“(2) The Carib Indians of the Islands, in consequence of their frequent excursions to the coasts of the *Tierra-firme* . . . . must have collected the germs of the pestilence which they would bring to their respective islands (the Little Antilles) so as to occasion new epidemics whenever they met individuals there in a state to contract the sickness.

“(3) The Island of Santo Domingo, except perhaps the Province of Higüey, which the Caribs were accustomed to visit, probably had not been invaded by the pestilence until 1495, the Admiral having been infected the previous year, 1494, on the coasts of Higüey.

“(4) The Isle of Cuba . . . . remained free from the invasion of yellow fever during the first 138 years of its settlement by the Spaniards, to 1649, in which year the pestilence was imported from the neighboring continent.”

Elsewhere he states that both Porto Rico, although it was much frequented by Caribs, and Jamaica were, he considers, free from yellow fever when first occupied by Spaniards.



Believed to be originally from the Amazon valley, the Caribs apparently had begun their war upon, and displacement of, the Arrowacks and Lucayans no great while before the discovery of America, and were extending it when the Spaniards came. They were bold navigators, going from the mainland up the chain of the Little Antilles to Porto Rico, Higuey (the south side of Santo Domingo), and Cuba. When first seen by the whites they occupied much, but not all, of the part of the Caribbean littoral of the mainland, "from the mouth of the Essequibo River to the Gulf of Maracaibo," says Brinton (1891, p. 251). "West of the latter" they "did not reach the coast . . . in spite of frequent assertions to the contrary." They occupied also practically all of the Little Antilles which were inhabited. Some of them were not inhabited at all when first visited by the whites, and a number not permanently. In Porto Rico and Hispaniola the Caribs occupied only parts of the islands, more of the first than of the latter, having apparently arrived only recently. They were savages, living in small communities and, on the Little Antilles at any rate, from hand to mouth. They cultivated maize and cassava to some extent, but supplemented them by wild fruits, roots, game, fish, shell-fish, and turtles. Many, I think not all, of them used poisoned arrows and were cannibals—indeed gave us this name.<sup>8</sup>

<sup>7</sup> If the part of Hispaniola (Hayti) occupied by the Lucayans contained 1,000,000 inhabitants, as asserted by Oviedo (1851-55, vol. 1, p. 71) this island, if not large enough to be added to the list of possible permanent foci, could have been an important factor in continuing the duration of the infection in the Carib area. One has, however, a general idea that the number of Indians defeated in battle or dying is exaggerated in these early accounts.

<sup>8</sup> The San Blas Indians, Brinton states, are not Caribs, but Cunas, which family extended from the Gulf of Uraba to the Chagres. They may be allied to the Caribs, or to the Chibchas of Colombia, but their affiliations are not certain. They did not use poisoned arrows.

## CHAPTER II

### MEXICO: RECORDS

Fortunately, we have, for Mexico, unusually complete records, both for a considerable period before the Spanish Conquest and for the century following it. The records available for this time may be classified into three groups:

I. The records of the Mexicans themselves, as kept before the Conquest in a conventionalized picture writing, hieroglyphics, and continued in the same manner for some time thereafter.

II. Those written soon after the Conquest by the Spanish historians who wrote of the Mexico prior to the Conquest, from native sources of information. These men were, in general, familiar with the language of the country (Nahuatl) and in intimate relation with the natives; and they made full use of Mexican knowledge, both record and tradition. They had, too, the assistance of natives "instructed in interpreting these pictures."

III. The writings of Spanish historians who wrote entirely from the Spanish standpoint, some from first-hand and some from second-hand information.

#### I. MEXICAN RECORDS

The period before the Conquest<sup>1</sup> is covered by the Nahuatl hieroglyphic manuscripts, now known as *codices*, and a number of *anales*, in that language, but written in Roman script. The first parts of these *anales* are evidently transcriptions from older hieroglyphics, some of which have been lost and some of which are attached to the *anales*. The latter parts are narratives of contemporaries and generally of eye-witnesses. In many of the *codices*, as we have them, along with the pictures are their interpretations by Mexicans "instructed in such matters," sometimes in Nahuatl, but in Latin script, and sometimes in Spanish. When the former is used, there are translations, generally literal, into Spanish or, in some cases, French. In a few, as in the *Anales de Tecamachalco*, which were not discovered, or at any rate not translated, early, there are Nahuatl words of which we do not know the

<sup>1</sup> August 13 (old style), 1521, when Cortez captured the City of Mexico, is generally accepted as its date.



meaning. These *codices* or *anales* cover not only the period before the Conquest, some from the beginning of the wanderings of the Chichimecs, the date of which is variously given, but continue for some time afterwards.<sup>2</sup> The *Codex Telleriano-Remensis*, for instance, is brought down to 1562 by the copyist (the original covers to 1555), and the *Anales de Chimalpahin* to 1612. There was less break in the general life of the Mexican people at the Conquest than one might suppose. The rulers of petty states continued to hold positions of authority under the Spaniards, and some of these *codices* were continued through and after the Conquest just as they had been before.

The history of the Mexican people, then, for some 75 years before the Conquest and for 50 to 60 years thereafter, is fairly well covered by these Nahuatl records. Naturally, they give more space to wars, territorial expansion, deaths and accession of rulers than to the data we especially seek; yet they do record eclipses of the sun, storms, earthquakes, drouths, famines, years of distress and of unusual mortality from various causes, etc. For the period during and after the Conquest, they continue their record of similar things. Thus, they note years of unusual mortality—two due to famines, and five at least to epidemic diseases. This native record since the Conquest we can compare with that of contemporary Spanish historians and thus get an idea of the point of view of the Nahuatl writers so as to measure the *negative* value of their records, that is, whether their failure to note an event means its non-occurrence, as well as their positive value. They measure up to this test nearly, not quite, so well as the Spanish writers of the first group; better than those of the second. These records naturally are authoritative on the customs of the Mexicans, on which they incidentally give much information, and are invaluable as a history of early Mexico. Their chronology, for the period named, and indeed for much longer, is reliable and has been verified by the dates given for eclipses of the sun.

The earlier records are, it is true, exclusively concerned with the Central Plateau, the "*tierra fria*," which was the origin and seat of the Mexican power, yet during the seventy-five years before the Conquest, which we have instanced as especially well covered by these records, this power was in communication with, and occupying parts of, the "*tierra caliente*," the possible original focus of yellow fever. The *Codex Teller-*

<sup>2</sup> We have what claims to be continuous history from that period, and fairly good, though scant, history from the latter part of the fourteenth century. Indeed, the *Anales de Cuauhtitlan* give an unbroken, although not a full, account from 635 to 1519.

*iano-Remensis* (pp. 38–42), for instance, gives 1458 as the year in which “the Mexicans . . . . subjected to their service the Province of Chicoaque . . . . towards the north, which is near Panuco. This is the first province which they subjected.” In 1461 “subjected the Mexicans the Province of Cotlaxtla . . . . twenty leagues from Vera Cruz.” This had again to be “conquered” in 1475. In 1502 the daughter of Montezuma, after she had borne children to the Lord of Tehuantepec, advised her husband that her father had given her in marriage to him to further designs on his kingdom, and “no more Mexicans entered that land until the Christians came, who subjected it.” Thus Tehuantepec, although not included in the Mexican realm, had been in close communication with it. In 1513 “the Mexicans conquer the lands of Totopec—80 leagues from Mexico, joined to the sea of the south” (the Gulf of Mexico, south of Panuco). They had long had communication, hostile and commercial, with the Mayas of the low country, some with those of Yucatan, but more with those of Huasteca, a province on the Panuco River, “as populous as any province under the sun,” says Fray Nicolas de Witte, who visited it in 1543.<sup>3</sup> The Huastecs were attacked and defeated several times (1460–1464) by Montezuma I, but not subjected (Durán, Diego, 1867–80, vol. i, p. 165).

I think we can claim, then, that if the *tierra caliente* was a permanent focus of yellow fever there was sufficient communication with it by the Mexican warriors and people from the plateau for yellow fever to show among them when they entered this region and for it to be carried into other infectible places to which they afterward went, and one would expect this to have been recorded.

## II. SPANISH WRITERS OF THE FIRST GROUP

Following the Conquest (1521) we have an unusual wealth of record by Spanish writers: Motolinia, who came to Mexico in 1524; Sahagún in 1529; Mendieta in 1554; Diego Durán, a Dominican, born in Mexico about 1538, who wrote about the middle of the sixteenth century, and finally, Torquemada. All were men familiar with the language. Sahagún’s great work was written in Nahuatl and translated by himself into Spanish, leaving an occasional word untranslated, a few of which we cannot yet find in the Nahuatl dictionaries. All were men of education, thoroughly interested in, and in sympathy with, their subjects. Also,

<sup>3</sup> Letter of de Witte, an Augustinian, written 1554, quoted by Ternaux-Compans (1840, vol. 16, p. 286).



they were all men with the kind of authority which brought them close to the natives,<sup>4</sup> and were in a position to know of what they were writing, having access to many Nahuatl picture writings not now available<sup>5</sup> and to Mexicans skilled in making and reading them. All of them give the sociological history of the people, not only the political and military events. Between them they give the history of the Mexicans from well before the Conquest to the end of the sixteenth century, their culture and customs before the Conquest, and those then prevailing. Sahagún, Durán, Mendieta, and Torquemada especially go into these learnedly and in much detail.

Torquemada was the official historian of his order, the Franciscans, for secular Mexico, as was Mendieta for the progress of the church therein. In his great work, *La Monarquía Indiana*, he evidently had the assistance of many others, Spaniards and Mexicans. Both he and Mendieta acknowledge their indebtedness to Olmos, Molina (authors of the first Nahuatl grammar and first dictionary, respectively), Motolinia, and others. Torquemada also mentions Orozco, Gomara, Acosta, and Mendieta himself, whose pupil he was. A large part of this book is taken up with the history before the Conquest, evidently from the Nahuatl picture-writing interpreted by educated Mexicans. He goes at length into the customs and culture of the Mexicans before and since the Conquest and also gives, in much detail, the history, both social and political, since the Conquest.

I will add that it is only comparatively recently that many of these works have been available (save in manuscript), as they were published since 1880. The same is true of a number of the *codices* and *anales*. We

<sup>4</sup> Motolinia was one of the founders of *la Ciudad de los Angeles* and held the royal appointment of *Guardián de los Indios* in Huexalzinco, and was most earnest, and most unpopular among the Spaniards, in the exercise of this function.

<sup>5</sup> Many Nahuatl manuscripts were purposely destroyed, as being idolatrous pictures, in the years soon after the Conquest. Archbishop Zumárraga is charged, by almost all writers on early Mexican history, with having had a great *auto-da-fé* of them—as did Bishop Landa at Mani in 1562 of the Maya manuscripts—and is much reprobated therefor. García Icazbalceta (1881, p. 305, 371) denies this most bitterly, and I believe that he is right. Certainly I can find nowhere the date at which any such special *auto-da-fé* was held, while we do know that of Landa's in Yucatan. There is no question, however, that there was a general intentional destruction of such manuscripts soon after the Conquest, for the reason given above, and that men of liberal culture, like Motolinia, Olmos, Molina, Durán, and Sahagún—the last especially—had seen and utilized many manuscripts not available to even their immediate successors.

have, therefore, advantages for this research not possessed by investigators of a generation ago.

### III. SPANISH WRITERS OF THE SECOND GROUP

The histories of Gomara, the chaplain to Cortez after his return to Spain, who afterward accompanied the conqueror to Mexico, of Bernal Diaz del Castillo, of Oviedo, and of Herrera, ordinarily quoted for early Mexican history, are decidedly less valuable for our purpose than those we have mentioned. The first two are primarily histories of the Conquest and give little information, even incidentally, on anything else. Gomara's account is intended as a eulogy of Cortez. He also was blessed with a vivid imagination and possibly valued the literal fact less than modern historians are supposed to.

The honest old Conquistador, Bernal Diaz del Castillo, confessedly wrote his book, *Historia Verdadera de la Conquista de la Nueva España*, to correct the many errors and faults in the book of Gomara, which he does in a wholehearted way. It is a wonderfully graphic account, and impresses one as an accurate report of the Conquest as he saw it—and he saw most of it—but of little else. He, I think, mentions sickness, as distinguished from wounds, only three times: once the epidemic of smallpox among the Indians in 1520, to be noted in its place; and, also to be noted in their places, that eight soldiers had died, apparently of pneumonia, at Saltocan the same year; and the deaths of three persons of note, possibly in a small epidemic of "*la modorra*," in Mexico City, in 1526. It is extremely well written, in spite of an absolute disregard of punctuation, spelling, and capitals—never once does he allow his leader a capital C!

Herrera was the official historiographer of Philip II and his successors, and his account was compiled from the reports of others. We probably now have more, and quite certainly more accurate, data than he had.

Much that Oviedo wrote was also from the reports of others, but he was much closer to his subject than was Herrera. He came to America, a young man, in 1514, and knew and talked with the men who had taken part in the things of which he wrote. His profession, that of a soldier, his official position as a *Veedor* (inspector) *del Rey* with the expedition of Pedrarias Davila to Darien, and his long residence in America should have given him a decidedly better knowledge of American affairs than seems possible to the official historiographer. He impresses one as being a far more acute man than Herrera, evaluating his data, not accepting them blindly. He is generally, and we think rightly, considered more



reliable than the latter. The first edition of his *Historia General y Natural de las Indias*, published in 1535, was the first systematic account of the New World to be published.<sup>6</sup>

<sup>6</sup> Oviedo has a certain sense of humor. After deprecating the idea, which he feared some of his readers might entertain, that an Indian "*naborio*" was in any sense a slave, since both a decree of the Emperor and a rescription of the Council (of the Indies) specifically denied this, he reluctantly admits that a "'*naborio*' is an Indian who has to work whether he wants to or not." I do not think that the official historiographer ever would have looked beyond the decree of the Emperor.

## CHAPTER III

### MEXICO: TIERRA FRIA

The foregoing lengthy discussion of our sources of information seems necessary in order that we may estimate the value of their information, especially the value of their *negation* of information. Neither for the period before the Conquest nor thereafter, up to and including 1576—for which time we have very complete data—is there, in either the Nahuatl records or in those of the first group of Spanish historians, an account of any sickness which can possibly be considered as yellow fever. There is a passage in Bernal Diaz which has been instanced, and an often-quoted one of Herrera which is held to imply the existence of this, or some similar, disease on the Mexican littoral about and to the north of Vera Cruz. They will be discussed later.

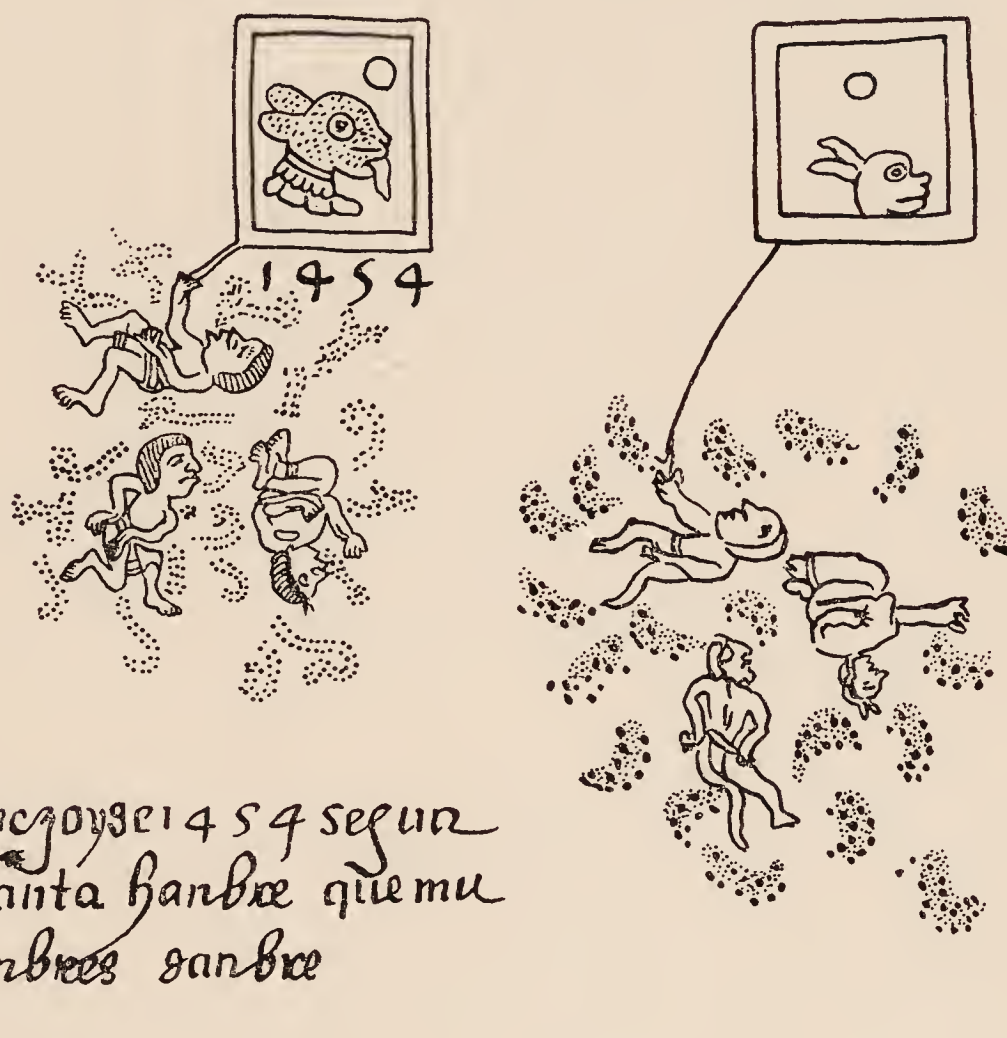
#### SICKNESS AND MORTALITY BEFORE THE CONQUEST

Before the Conquest, some years of unusual mortality (as 1447, 1452, 1453, 1454, and 1506) are noted. In every instance the cause is given as either cold or famine, and this famine *preceding* the mortality. In every instance, too, a sufficient cause is given for the famine: grasshoppers in 1446; drouth in 1504 and 1505; or unusual cold, with hail or snow, destroying the crops. This statement of cause is the more convincing when we consider that these records were made year by year as the years passed, and were not retrospective. And in every instance the mortality is recorded as occurring in the *tierra fria* where, often introduced, yellow fever has never been known to spread, it being too cold either for sufficient *aegypti* activity or for the development of the causative organism in the insect. Whatever the cause, the historical proof of this region's being "uninfectible" by yellow fever in historical times, is complete. It may be noted, too, that, save the last, all of these instances of unusual mortality occurred before the arms of the Mexicans were carried into the coastal regions about Vera Cruz or Panuco. After that, about 1458, we hear of no special epidemic for some fifty years. This is contrary to what one would expect had this *tierra caliente* been a permanent endemic focus of yellow fever at this time.

There is, in this period, one year of very great mortality, 1454, which deserves special mention, as the Nahuatl picture accompanying that



year has been interpreted as showing an epidemic of yellow fever. This picture, in the *Codex Telleriano-Remensis* (f. 38), represents three figures, two men and a woman, one man and the woman vomiting, and all apparently dying. The material around them, apparently vomited, resembles worms or snakes and is of the same brown tint as the men, but delineated by black dots, instead of continuous black lines as the figures themselves are. The Spanish text interpreting this picture, by "*Preste Juan de las Yndias*," who "found the jewels" (this



Ano de vn conejose 1454 segun  
 sanca vno tanta hanbre que mu-  
 rian los ombres sanbre

FIG. 3. RECORD FOR THE YEAR "ONE RABBIT" (1454), AS DEPICTED (LEFT) IN *Codex Telleriano-Remensis*, AND (RIGHT) IN *Codex Rios (Vaticanus)*

Codex) "in the year 5 Rabbit," i.e., 1562, is: "Year 1 Rabbit, and of 1454 according to ours, there was so great hunger that men died of the hunger." This picture, however, shows sickness, and sickness of which vomiting, possibly with occasional worms (*ascarides*), was a prominent feature. There is no evidence anywhere of blood, which, in this Codex, both before and after this picture, is always represented with well marked splotches of red. The action of the picture was on the highlands of Mexico, about 8,000 feet above sea-level, where yellow fever is out of the question.

The "calamities" of this and the immediately preceding year are noted in a number of other records: the *Codex Aubin* (plate 69),<sup>1</sup> the *Anales de Chimalpahin* and of *Tecamachalco*, the histories of Sahagún<sup>2</sup> and of

<sup>1</sup> It is always well to consult original documents, and when this is impossible one is at a disadvantage. There is ever a chance of error in quotation or transcription, and even slight errors may be important. The original pre-Columbian manuscript, of which the *Codex Telleriano-Remensis* is a copy, is not available; it is quite certainly destroyed; but there is another copy, the *Codex Rios* (*Vaticanus* 3738), a photostat of which is accessible. Both are evidently made from the same original and about the same time, and both are illuminated in the same colors. The *Vaticanus* has no explanatory text with its glyphs, nor Arabic numerals to indicate the years; both of which it had certainly had if it had been copied from the *Codex Telleriano-Remensis*. It was apparently copied in 1564, and the *Codex Telleriano-Remensis* in 1562, from a manuscript which ended in 1555. There is one phrase in Latin in the *Vaticanus*, under the glyph for Tenochtitlan (whether by the copyist or added later one does not know) which is not in the *Telleriano-Remensis*. The glyphs for this year, "1 Rabbit," in both are essentially the same, but the differences, though slight, are, for us, important. In the *Vaticanus*, none of the figures are represented as vomiting—all have their mouths shut; and the material which I have indicated as vomit is not especially like worms.

I am much inclined to think that "*Preste Juan de las Yndias*" made the more accurate copy, yet there is no relation between what, to us, is the distinctive feature of his drawing and his own (Spanish) text. Had we had only the *Vaticanus* there had never been any claim that this drawing represented an epidemic characterized by vomiting.

We may note also in the *Codex Aubin* (plate 69) that for the previous year, "13 House" (1453), a hail storm is shown beating upon the corn; and the corresponding entry in Nahuatl states: "the famine commenced." The year "2 Reed" (1455) following the year "1 Rabbit," shows a heavy mortality—a bird eating a corpse *in the open*—but nothing to indicate an epidemic.

Is it possible that *Preste Juan* was influenced by the tradition of the "*mayacimlal*" in Yucatan about 1485? The tradition of it must have been known to the Mexicans of his time, and the gruesome and unusual seems to have a fascination for people in a certain stage of culture. He may thus have made his drawing show his people die vomiting worms instead of simply dying as does the drawing in the *Vaticanus*, but I am rather unwilling to believe it. One would like much to see the original manuscript, the "Jewels" found by *Preste Juan* in 1562.

<sup>2</sup> Padre Sahagún (1829-30) vol. 2, p. 269) says that this four years' famine was called "*nocetochviloe*," one of the words he left in the original when he translated his original text from Nahuatl into Castilian. His editors, both Spanish and French, have passed it by, giving only the word as Sahagún first penned it; nor is it, in its completeness, in any Nahuatl dictionary available to the writer. Yet its root seems so plainly to be: "*ce*"—one, and "*tochli*"—rabbit; and its form as clearly adjectival—"in or after the manner of"—that the writer is much inclined to render it accordingly. This year, 1 Rabbit, as it recurred every 52 years in the Nahuatl chronological cycle, was regarded as extremely unlucky and accom-



Torquemada, all of which give a clear history of three or four years of unusual cold which, with hail or snow beating down the corn, destroyed the crops, and from this came the famine from which so many died. Even the *Codex Telleriano-Remensis* gives: "year of 7 Reed, 1447 according to ours, there were so great snows that men died," and pictures the snow or hail and dying men. To quote Torquemada (vol. 1, pp. 158-59), who gives as his authority: "This affirm the histories and paintings of that time . . . . There was an almost universal hunger in all the *tierra fria*, because, when the grain was in milk, great hail fell one time after another (*vnos Dias tras otros*) and broke it all down, and this year they gathered no maize." (The *Anales* say: "not much they harvested.") "They lived this year on last year's supply without much hunger." The following year "there came again Ices"—hail storms doubtless—"which beat down everything and there was great hunger in the land." The next year "there fell no rain from heaven, and the sun scorched the earth nor, because they had no grain to plant and it was cold, did the fourth year lack of being also very hurtful. There resulted then at this time a great hunger and the people ate wild roots and unclean things in the woods, and many died, and they sold their children for maize . . . . a damsel for 400 ears of maize and a youth for 500 ears." Torquemada adds that it was from the Totonecques that they bought maize and to these that they sold their children.

This famine, which had lasted more or less from 1447 (grasshoppers preceded the cold), was followed by a year of much rain and great abundance. All agree on this. Chimalpahin also notes for 1456: "year 3 Knife . . . . great abundance . . . . pestilence reigned severely." No one else notes this pestilence. It may have been either a pestilence localized to one district, or an error.

Father Torquemada is led to marvel at this reverse, in Mexico, of the condition which had prevailed in Egypt in the time of Joseph, where the lean years had followed those of great abundance, instead of preceding them, and is decidedly critical of the Mexican method as compared with the Egyptian.

It is entirely true that the search for food, necessitated by a failure of crops caused by cold, almost inevitably would have brought about communications with warmer regions, as was the country of the Totonecques,

---

panied by all kinds of generalized misfortunes (see page 102). This word "*nocetochviloe*" might, then, be translated: "after the manner of 1 Rabbit;" that is, "disastrous" or "hurtful," the word used by Torquemada.

where the crops had not failed,<sup>3</sup> and hence there would be exposure to, and importation of, such communicable diseases as might exist in the regions visited. If yellow fever existed in these warm countries, it would, or might, have been contracted there by some of the men from the plateau going down. Some, travelling rapidly, might have developed it on the plateau; but, unless either the climate of the valley of Mexico or the biology of *aegypti* or of the causative organism of yellow fever has undergone marked change since 1454, the disease would not have propagated itself there. It has been introduced very often in more modern times and has never done so. This epidemic in these localities could not have been yellow fever.

#### SICKNESS AND MORTALITY AFTER THE CONQUEST

During the period of, say, fifty to seventy-five years after the Conquest, there are recorded a number of epidemics, mainly or exclusively of diseases introduced from Europe. Some of these are designated only by their Spanish names, or the Nahuatl equivalents; others are both named and described, and described as well as similar things elsewhere were described at this time. All of the historians of our first two groups mention some of them, the general epidemics, but as they lived in different regions, they do not all mention the more circumscribed epidemics. Mendieta gives, in general, the fullest account. He came to Mexico in 1554 (García Icazbalceta, 1886-1892, vol. i, p. xi) but states that on the subject matter of his book he received much instruction, orally and in manuscript, from Padres Andrés de Olmos and from Motolinia, who was appointed his guardian. These men had come in 1528 and 1524 respectively. His is, then, practically a contemporary record from the Conquest to 1596, when it was finished during a mixed epidemic of measles, "*paperas*" (a swelling of the neck, either mumps or diphtheria), and "*tabardillo*" (typhus).

An abbreviated account of the epidemics recorded in Mendieta's *Historia Ecclesiástica Indiana* is given, with such interpolations from other sources as seem necessary. When no authority is given Mendieta is meant (pp. 514 *et seq.*).

*Year 1520.* Smallpox was introduced into the City of Mexico by a

<sup>3</sup> Sahagún (1880, Jourdanet's translation, p. 669) says of Totonaca (I judge the country of the Totonecques, although he places the Guastiques there): "The heat is very strong in this country—no cacao is raised," but "Spanish fruits are very common there." Brinton (1891, p. 139) places it in the present State of Vera Cruz.



negro in the train of Narvaez, not long before the "*Noche Triste*" (July 10, 1520), when Cortez had to abandon the city. The epidemic did much to break the Mexican resistance against Cortez' return. It spread generally and gave a fearful mortality. All, even Bernal Diaz (1904, vol. i, p. 409), note this.

*Year 1531.* Measles prevailed, widespread and, to the good Father's astonishment, with a very heavy mortality, but less than that of smallpox. The translator of Chimalpahin (Rémi Siméon) calls this "smallpox, which . . . made die the little children" (Chimalpahin, p. 225), but Motolinia (pp. 13-14) agrees with Mendieta in calling it measles, "*sarampion*."<sup>4</sup>

*Year 1538.* "Year of 7 Rabbit—died many Indians of smallpox." Figured in *Codex Telleriano-Remensis* and figure interpreted by "Priest John of the Indies;" not given by Mendieta.

*Year 1545.* "The third pestilence" (Mendieta does not note that of 1538), "great and general, came in the year 1545 . . . . This was of a pushing out (*pujamiento*) of blood, joined with fevers, and so great was the blood that it gushed forth from the nostrils." This pestilence is noted by all of our historians, Spanish and Mexican, except Motolinia, whose book stops in 1544. The *Codex Telleriano-Remensis* figures it as in both 1544 and 1545—dead men and women lying in a large pile. Chimalpahin gives: "The year 1 House" (1545) "there was death; blood by the mouth; the eyes; the nose; the anus. Then perished extremely" (many) "nobles, men and women: also common people. Then dogs and jackals" (coyotes?) "fed on people in Chalco." Torquemada (vol. i, p. 643) states 800,000 died in Mexico. Mendieta gives 150,000 in Tlaxcala and 100,000 in Cholula, and other places in proportion. Sahagún (1829-30) says: "In that part of the City of Mexico called Tlaltelolco, where I was then residing, I buried more than 10,000 corpses and, towards the end of the epidemic, was myself attacked and near to dying."<sup>5</sup>

<sup>4</sup> The first word used for smallpox, in Mexico, in 1520, was "*tohtomonaliztli*"—"to have pustules." After this it appears as "*huey çahuatl*," "great pocks" or eruption, and measles as "*tepiton çahuatl*," "little pocks" or eruption. The word used by Chimalpahin for this epidemic is "*çahuatl*"—"the eruption," without any adjective. That it was especially fatal to young children is distinctive and adds evidence of the careful observation of our Mexican historian.

<sup>5</sup> In Sahagún's *Historia*, both in the Spanish edition (1829-30) and in the edition translated into French and edited by Jourdanet and Rémi Siméon (1880), this date is given as 1555 (*lib. xi, cap. 12*) although it is implicitly corrected to 1545 further on in the same chapter and explicitly in chapter xii of the same book (page 792, French edition, 1880; *tomo 3: p. 334*, Spanish edition, 1829-30).

*Year 1550.* "Many Indians died of *paperas*" . . . swelling of the neck and face, the Spanish word used for mumps (*Cod. Tel.-Rem.* p. 44). Chimalpahin (pp. 246-47) gives: "Year 6 Rabbit (1550) . . . malignant anginas" (literally "close-embrace-around-the-throat-death") "made many victims." Was it diphtheria?<sup>6</sup>

*Year 1558.* "Year 1 Rabbit—they will see as ever that there has fallen" (in this year) "a heavy hunger and mortality and thus in this year happened the greatest ices" (hail storms?) "which the natives remember and scarcity in some parts." Thus the *Codex Telleriano-Remensis* (p. 44). No one else gives it. (It has already been noted that this year of 1 Rabbit, when it returned on the cycle, once every 52 years, was considered extremely unlucky and greatly dreaded for famines, etc.)

*Year 1563.* Chimalpahin (p. 257): "Year of 6 Canes, 1563—reigned a pleurisy, which struck and destroyed a very great number of subjects and Seigneurs." The word translated "*pleurésie*" is "*matlaltotonqui*," and the translation is Rémi Siméon's. Yet, bold as it seems to say it, this translation is clearly an error. In a note to Sahagún's description of the great epidemic of 1576 (Sahagún, 1880, p. 877), Rémi Siméon translates this same word, used by the same writer, Chimalpahin, as "*fièvre verdâtre*" (bluish-green fever); and this is clearly correct, since his own dictionary, as do others, gives "*matlalli*" as "*verdâtre*" (bluish-green); and "*totonqui*" as "heat: fever." This was quite certainly the same disease as prevailed in 1544-45 and in 1576-77, and, as on these occasions, it seems to have lasted into two years, prevailing through the winter.

*Year 1564.* The above was probably the same epidemic which, by Mendieta, is ascribed to 1564, going over from one year to the other. "This was a year of high mortality," says Mendieta of 1564. This disease was not named or described by Mendieta, but was ascribed by him to a census of the Indians, taken by Valderrama—a thing which he states had likewise produced a pestilence in King David's time.

*Year 1576.* Mendieta: "Came another pestilence, of which died a very great number of people in all parts. This was a pushing out (*pujamiento*) of blood like the others,<sup>7</sup> and showed as typhus (*daba en tabardillo*)."

<sup>6</sup> Villalba (1803, vol. i, p. 90) states an epidemic of "*garrotillo*" was prevalent in Italy and Spain in 1530, brought into Europe from Astrakhan.

<sup>7</sup> The use of the plural, "others," looks as if he included the "*mortalidad*" of 1564 as being of the same class with that of 1544-45 (as from the designation of Chimalpahin it was) and with that of 1576-77.



Chimalpahin (trans. by Rémi Siméon, pp. 288–292) gives: “The year 6 Knife”(1576) . . . . “Then began in the country of Amaquémécan-Chalco the sickness called ‘*cocoliztli*’ (epidemic); it began in August and was not very strong; but it was above all extremely grave in the month of September when it raged 30 days; whilst the great epidemic lasted reigned the *matlaltotonqui* (*pleurésie*); blood came out without effort by the mouth and by the nose and brought death; there was no remedy, also, great numbers of people died in all New Spain . . . . Then it froze during XV days of the said month of September and that continued during XXXI days of October; the mortality reigned at the same time.”<sup>8</sup>

The epidemic lasted into the next year: “Through XXXI days of January, XXVIII days of February and XXXI days of March continued the epidemic . . . . after that time the evil diminished a little.” The *Anales de Tecamachalco* (p. 64 *et seq.*) also give the names of a number of Spaniards, “*personas de consideración*,” dying of it in 1577—one on February 11—and the disease was evidently still prevalent at that time. Torquemada (vol. i, p. 643) gives the mortality of this epidemic as 2,000,000—an estimate, of course, as was that of 800,000 for the epidemic of 1544 and 1545.

*Year 1588.* “From a scarcity of maize many died.”

*Year 1595.* “At the end of the year 1595 and the beginning of 1596, at which time I write, there reigns a most general pestilence mixed of measles, *paperas*, and *tabardillo* (typhus) that scarcely a man remains on his feet,” says Mendieta (p. 515), . . . . “although less mortal than the previous ones.”

I have not pretended to note every writer who mentioned each epidemic. All mention the general ones which occurred in their times, and most of them mention some smaller local ones, which I have not given. I think, however, that the eight epidemics and the two years of “*mortalidad*” from general failure of crops, in 76 years—and there were other local famines noted—show that our historians were awake to the consideration of such things, and that up to 1576, at any rate, when nearly all of them were recording these epidemics (six in 55 years after the Conquest), any considerable outbreak of mortality among either Spaniards or Indians had been noted. It was to bring out this, their *negative* value,

<sup>8</sup> The word rendered by Rémi Siméon as “*gela*” (froze), about which there might be question is “*cehuitzico*”—compounded, apparently, of “*cecuiztl*,” cold, and “*tzicoa*,” to fix solidly, hence “*cold fixed solidly*”—i.e. frozen. Compare this and the year 1558 with 1454, in which deaths are also ascribed solely to famine without an epidemic.

as evidence that an epidemic not noted did not occur, that this matter has been studied so extensively.

As to these epidemics, the only question is as to the nature of those, quite certainly the same, of 1544–45 and of 1576–77, characterized by such high mortality and abundant hemorrhages through the mouth and other orifices of the body. Considering the locality—the *tierra fria*—and the time of the year—lasting through the winter—it seems not worth while to discuss the possibility of yellow fever, even if we had not mention of the unusual cold in the account of Chimalpahin and others; not more than in the epidemics with similar symptoms in Dublin, in the winter of 1826–27, and in Dundee and Glasgow, in 1843. Mendieta, I think, settles the question. He unquestionably considers the epidemics of 1544 and 1576 the same, and from the use of the plural “others,” implies that there had been at least *two* of the same kind (1544–45 and 1563–64) before 1576. “*Tabardillo*” was, in Spain at this time, the current name for typhus fever, and possibly sometimes for scarlet fever, probably on account of its eruption; and when Mendieta said “*y daba en tabardillo*,” he quite certainly meant the typhus eruption. So, in his account of the mixed epidemic of 1595–96, is his use of the same word “*tabardillo*.”

*Probable typhus, 1526.* The deaths of the Licentiate Luis Ponce de Leon, of Padre Ortiz, and of young Aguilar, recorded by Bernal Diaz (1904–05, v. 2, p. 362–365) as dying in Mexico City of a “*modorra*,” in 1526, were quite certainly due to typhus. Cortez (1866, p. 482) gives an account of this matter in a letter to Charles V, under date of September 3, 1526: “. . . and in this time the said Luis Ponce . . . fell sick and all as many as came in the armada in which he came. Of which sickness it pleased Our Lord that he should die and more than thirty others of those who came in the armada, among whom died two friars of the order of Sto. Domingo, who came with him, and up to today there are many persons sick and in much danger of death; for what they brought with them has appeared almost like a pestilence” (i.e. a communicable disease) “for it attacked even some of those who had been living here (*que acá estaban*) and two died of the same sickness and there are many others who have not recovered from it,” that is, are still sick.

Ponce landed at Medellin, two leagues from the port of San Juan de Ulua, and came direct and “without delaying” to Mexico City. The date of his landing we do not know, nor the date of his arrival in Mexico City, nor the date of his falling sick, nor that of his death. This is rather usual in Spanish chronicles of the period. *What* happened was of more



importance to them than *when* it happened. The writer was surprised to see that the Cortez letters were dated.

The messenger, announcing Ponce's arrival at Medellin, reached Cortez in Mexico City on June 25, ("the day following St. John's Day"), and Ponce followed him closely. He stopped when two leagues outside of the city to communicate with Cortez, and lay there until the next day. He presented his credentials two days later. The beginning of his sickness is given by Cortez (p. 371) in another letter (September 11, 1526) as occurring in the night after this day ("*aquella misma noche*"). The same letter says that he died "eighteen or nineteen days after his arrival in the city." Either, then, he was sick sixteen or seventeen days or, if he died on the ninth day of his illness, "lying somnolent, passing four days, night and day, in sleep," as Bernal Diaz (1904-05, vol. 2, p. 362) states, he had been in the city longer than Cortez says before the attack. The first is much the more probable. The date of his attack, then, would have been near the end of June or in very early July; and the sickness alleged to have been introduced by him lasted, according to Cortez, some two and a half months at least.

Cortez was fifteen days en route from Medellin to Mexico, his journey being made about ten days earlier. There were reasons for haste, and he was a most energetic man, as indeed was Ponce. Even if Ponce had covered the distance in half of this time, the onset of his attack had been at least ten days from leaving Medellin . . . well past the incubation period of yellow fever from possible exposure there, but, even if he took as long as Cortez, within that of typhus. If this were typhus, as seems not improbable, this was quite certainly its earliest introduction from Spain into Mexico of which we have any knowledge. It is fair to say, however, that as Cortez was accused of poisoning the Licentiate at the banquet, the date of which apparently determined the date he gives for the onset of the illness, he may have laid more stress on the ship-origin of this sickness than he would otherwise have done. Still, he agrees fairly well with Bernal Diaz—as far as the latter notices this sickness—and I am inclined to accept his basic statements, namely, that there was a small epidemic among the Spaniards in Mexico City soon after the arrival of Ponce, beginning among those who came with him and later attacking some who had been residing in that city.

It, I think, was almost certainly typhus. Only the Spaniards are reported as having it, and there was evidently no great spread among Indians: a small spread would not have been noted. Possibly its vector, the body louse, was not indigenous to America and was not sufficiently disseminated, even in the City of Mexico, at this time, only five

years after the Conquest, for an epidemic among the Mexicans. There was time for a greater dissemination by 1544, the date of the first epidemic (of "*matlaltotonqui*"), mainly in and about the City of Mexico, and for a general dissemination on the plateau by 1576, when the great epidemic occurred.<sup>9</sup>

*The epidemics of 1544-45 and 1576.* "*Matlazahuatl*." Humboldt (1811, vol. i, p. 352 *et seq.*) calls these epidemics of 1545 and 1576, along with a number of subsequent ones, "*matlazahuatl*," apparently on the authority of Torquemada, although he does not quote him directly. I can find this word in no Nahuatl or early Spanish writer—although, as there are nearly 3,000 folio pages in Torquemada's *Monarquía Indiana*, and no index, it may have been overlooked. It is called "*peste*," "*pestilencia*," "*mortalidad*," "*epidemia*," "*cocoliztle*," and "*matlaltotonqui*" by them. This word "*matlazahuatl*," however, is used by Rémi Siméon in a note of his own at the end of his translation of Sahagún's *Historia* (p. 877) as if it were well known as applicable to the epidemic of 1576-77; and from its meaning he judged that "the spots the disease produced had a bluish-green (*verdâtre*) aspect."

In spite of the statement of Humboldt (1811, vol. i, p. 344-352) quoted by, I think, every writer who has touched on this subject since his time, that this disease was absolutely confined to Indians and the dark-skinned of the mixed races, many Spaniards died of it. Torquemada says: ". . . even of the ministers who confessed them many accompanied them in death, because as they were continuous in ministering to them, the sickness (which was contagious) struck them and in a few days died many of the brothers of St. Francis, who perished in this work." In the *Anales de Tecamachalco* (p. 64 *et seq.*), probably twenty Spaniards, "*personas de consideración*," are mentioned by name as dying of it in one district. Sahagún, however, (*vide infra*) certainly implies a greater destructiveness to Indians than to Spaniards. To typhus, which had been very prevalent in Spain since 1557, and endemic since 1490, a considerable number of Spaniards should have been immune by previous attack.

This "*matlazahuatl*" has always been surrounded with mystery, some considering it yellow fever; but more generally it was believed to have been a disease indigenous and peculiar to the Mexican highlands and to which Europeans were not subject. If there was such a disease, from time to time epidemic there, characterized by vomiting of blood and

<sup>9</sup> See also Prescott (1873, vol. iii, p. 292); Jourdanet, note to his translation of Bernal Diaz (p. 744); and note to page 337 of McNutt's (1908) translation of this same letter of Cortez.



other hemorrhages, it has disappeared, as the only endemic diseases—from time to time becoming epidemic—prevailing in the highlands now are smallpox and typhus fever, the latter known by the same name as in Mendieta's time, "*tabardillo*," and apparently identical in symptoms, epidemiology, and vector with the typhus of the Old World, from which the writer believes it to have been introduced.

Moreover, the word "*matlazahuatl*" clearly implies an eruption. Smallpox was "*hueyzahuatl*"—the large pocks or eruption; measles was "*tepitonzahuatl*"—little pocks or eruption; and this disease, "*matlazahuatl*," could only be the "*matla*," pocks or eruption; and the dictionaries of Rémi Siméon, Molina, Biondelli, and the Licentiate Robelo, all give the root of this word (*matlal*) as meaning a color—not always the same color, however.

Mendieta unquestionably believed that he was dealing with typhus, and the history of the introduction of typhus into Mexico is definite, we believe, in 1526; surely in 1570. That typhus, as had been measles and smallpox, would be much more malignant among a race virgin to it, and in a community almost entirely susceptible, is very probable. It is now severe enough, but no more so in Mexico than in Europe and North Africa. Or one might hazard a guess, and not an improbable one, that not only was the epidemic of 1595–96, but those of 1544 and 1576 as well (we have no account of hemorrhage, nor indeed any description of the disease in 1563 and 1564), mixed, of typhus and relapsing fever, such as occurred in Dublin in the winter of 1826–27 (Graves, 1848), and in Dundee and Edinburgh in 1843 (Arrott, 1844; Cormack, 1843; Craigie, 1843; and David Smith, 1844). The two diseases have the same vector, and in Europe were frequently associated, as, indeed, they are now (1923) so reported in Eastern Europe. I have no explanation of the deaths characterized by vomiting, apparently of worms, figured in the *Codex Telleriano-Remensis* as occurring in 1454, unless it was that this was a usual result—or at least one that impressed the historian—from the use of improper food, due to the "hunger" that prevailed. Diarrhea rather than vomiting, although both occurred, was a common terminal symptom of starvation among the Cuban *reconcentrados*, as the writer knew of it in 1899. Profuse vomiting, however, from any cause, in people living under primitive conditions, is not unusually accompanied by the vomiting of worms (lumbricoids). Père Labat (1722, vol. i, p. 73) gives as part of his description of the "*mal de Siam*" of 1694: "*quelque fois on rendoient paquets de vers de differents grandeurs & couleurs.*"

Padre Sahagún—a good man and a strong man—completed his book during the epidemic of 1576, and his account (vol. iii, pp. 328–335) is so

human a document and appeals so strongly to all who, like the writer, have seen and felt the agony of a plague-stricken city, that one can scarcely help giving some of it:

“But alas! in this year of 1576, in the month of August, an epidemic as universal as great began. It has lasted three months: many are already dead and each day more die. I do not know how long this one will last or what will be the extent of the evil. I am now in the City of Mexico, in the part called Tlaltelolco, and I see, from the moment that it began up to this day, the 8th day of November, the number of dead increases day by day, from ten to twenty to thirty to forty to fifty to sixty to eighty. I do not know what it will be in the future . . . Many will die of hunger and of having no care. It has happened and is now happening, in many houses, that all of them fall sick without leaving one who can give them a cup of water. And in order to administer the sacraments in many places there is no one to come to the church nor to tell us they are sick. When this is known, the monks walk from house to house, confessing and consoling them. When this pestilence began . . . the clergy and monks, as of St. Francis, so of St. Dominic and of St. Augustin and the Jesuits, walked from house to house to confess and console them, and this lasted for two months and then ceased entirely, because some were tired and some were sick and others occupied on their *haciendas*. Now already many priests fail to come, those who were aiding and those who were not aiding.

“In this pueblo of Tlaltelolco only the brothers of St. Francis are now walking from house to house confessing and consoling them and giving the bread of Castile” (wheaten bread) “to eat, bought by our own alms, and that seems about to stop, for bread is very dear and the Brothers go sick and tired, for which there is a great tribulation and affliction . . . Please Our Lord to remedy this so great plague, because, if it lasts much longer, all will be finished.”

He tells, just as he ends the book, of a prophecy that all the Canary Islanders and all the Indians of New Spain are to die within seventy years of the coming of the Spaniards. “This prophecy I had not credited, but the things that have happened, and are happening, appear about to prove its truth” . . . “for if this business, which now is, goes forward three or four months as it now goes, there will remain no one (*no quedará nadie*).” Evidently the effect of the disease was mainly confined to the Indians, for he adds: “It seems that the race will be entirely destroyed and the land left to wild beasts and forest. It would be then uninhabitable, there not being enough Spaniards in it to hold it.”

In the last paragraph of the 11th book, with which the *Historia* really ends, his faith revives, and he believes “that the epidemic will cease soon



and that there will remain enough" (Indians) "to give the Spanish time to multiply and colonize the country, so that, the one race lacking"—still the prophecy—"it will remain peopled by another, that is the Spanish, and I even hold that there will be always a quantity of Indians in these lands." Ending his work with this hope—which time has justified.

*Scant records of sickness by Diaz del Castillo.* Taking up the second group of Spanish historians, the only reference I find by Bernal Diaz to any sickness, besides the smallpox among the Indians in 1520 and the deaths of three prominent people in Mexico City of the "*modorra*" in 1526, to which we have alluded, and the *mal de buas* (venereal disease), is the following:

After his disastrous retreat from Mexico City, when Cortez was at Saltocan, six leagues distant therefrom, "he saw many of our soldiers wounded and suffering and that there had died eight of pain in the side and of throwing off clotted blood mixed with mud by the mouth and nostrils and this was from their being broken down by the arms which we ever carried and because of the continual going along the trails and the dust which we swallowed of necessity."<sup>10</sup> This probably was pneumonia. Considering the locality and that they had been living in Mexico City for a long time, it was not yellow fever.<sup>11</sup>

<sup>10</sup> *Se avian muerto ocho de dolor de costado y de hechar sangre quaxada rrevuelta con lodo por la boca y narizes* (Diaz del Castillo: 1904-05, vol. 2, cap. 140).

<sup>11</sup> Dr. M. E. Connor, of the International Health Board (personal communication), had a case or cases of pneumonia presented to him in the summer or fall of 1923 in Mexico for yellow fever, on account of the bloody sputum. The writer is quite sure that the great epidemic of 1759, "during the time of ices," noted by Ulloa (1772, p. 200) in the Andean Plateau, which, with others, maybe of a different nature, gave rise to the reports of yellow fever at Quito, Cuzco, and Cerro de Pasco, was an epidemic of influenza-pneumonia. Dr. Unanue's description, of which I have never seen mention, makes this very clear to me. He calls it "a malignant catarrh," and gives as symptoms "excessive prostration, pain in all the body, especially in the costal regions, spitting of blood, rapid respirations, and little fever" (Unanue, 1815, pp. 25 and 124).

This is quoted, indeed, for the description of the epidemic of 1720, on the same plateau, characterized as "the most fearful pestilence that ever devastated Peru," but Dr. Unanue at this point states that "this pestilence of 1759 at Lima was the same as that of 1720 inland."

Ulloa (*loc. cit.*) gives nearly the same description as above, but adds: "blood by the mouth and the nose." He states that it was very general. "Had it been as fatal as universal the race had been exterminated." He adds that it appeared very suddenly, coming up from the south, spread with extreme rapidity and disappeared the same way, and that the great comet of 1759 also came up from the south and did the same way.

Most of these Andean epidemics, however, including that of Quito and Cuzco of 1720, have, in the opinion of the writer, been typhus.

## CHAPTER IV

### MEXICO: TIERRA CALIENTE

#### HERRERA'S ACCOUNT OF UNHEALTHFULNESS

All of the epidemics hitherto noted have been in the Mexican plateau, and it may be that the writer has wasted time in discussing them. Herrera, however, although writing at Madrid at a somewhat later date (published first in 1601) and from the data of others, pays attention to the low country, especially the coast about Vera Cruz—of which city he is in nowise an admirer.

“It rains from April to . . . September,” as it still does. “There is no running water,” hence water would be stored, “and the rain sinks in the sand.” During this season “the strong sun draws out the dampness and with the very sultry weather places it in the nostrils whence it appears that it pierces to the brain and gives fevers (*calenturas*) which very few escape; clearly it is seen that this vapor is poisonous, because even the Plaza of the city is made a marsh in the time of rains and gives birth each morning to thousands of little animals . . . with tails and with feet, and in a few days the tails fall off and they show as great toads giving at night frightful voices. Moreover, it is close to a river which, although good water and drunk daily is made” (as hot as) “broth and to the people going out in the sun . . . these bad vapors and the hot waters give sicknesses; and the same in all the Coast of the North from Panuco to Guazacoalco” (Coatzacoalco?). “From November to March there is no rain and strong north winds make it cold and then, when the land is dry, it is as healthy as Mexico” (Herrera, 1726–27, *dec.* iv, *lib.* ix, *cap.* vi).

There is nothing in this account enlightening us as to the *nature* of the sickness at Vera Cruz and along the coast. The implication seems rather that it was very prevalent than that it was very deadly. The physical conditions were favorable for both malaria and yellow fever. Unquestionably, in addition to the abhorred batrachians, the plaza bred *Anopheles* mosquitoes—probably *albimanus* as now—and, with the introduction of human carriers from Spain or Africa, which had surely taken place before Herrera's time, malaria would be prevalent in Vera Cruz and in much of this coast. Also, although close to a river of pota-



ble water, the conditions were conducive to storing water, and hence, if the insect vector of yellow fever and its causative organism were there, this disease would propagate in proportion to the number of susceptible people supplied. Vera Cruz was the port of entry and of departure of Spaniards to and from the Mexican plateau, and the heavy immigration soon after the Conquest might, and probably would, have been sufficient to continue yellow fever in permanence here. Whether Indians from the plateau were brought to the port for labor soon after the Conquest, as they come now, we have no knowledge.

The paragraph of Herrera, however, which is always cited in evidence of the presence of yellow fever on this coast before the Conquest is the following (*op. cit.*, *dec.* iv, *lib.* ix, *cap.* viii).

“Of the sickness called *Cocoliztle* and for which cause the Coast of the North of New-Spain is depopulated . . . . I have already said that the City of Vera Cruz is sickly and all the Coast of the North, being a hot country where all diseases are more fatal because the heat of the region adds itself to the natural heat” (of fever?) “and does not allow the sick man to recuperate because the hot air prevents him, and children are not raised because with any kind of a disorder a fever supervenes. For this cause the coast is found depopulated and the reason why it had so many people in the time of Montezuma is that, although it had the same diseases prevalent, which they call *Cocoliztle*, and in some years more than others, as is the case now, Montezuma was accustomed, in view of the mortality and lack of population in those lands, to draw from Mexico, and from other pueblos where there were many people, eight thousand families, and this number of eight thousand families they called ‘*zexiquipil*,’<sup>1</sup> and sent them to people where there had been a great *Cocoliztle* and gave them houses and lands and made them free of tribute for many years, and thus he returned to re-people the coast whenever there was need, without making any lack in the pueblos whence he drew them, and thus they give the name *Cocoliztle* to the smallpox when generally prevalent . . . and to other universal deadly sicknesses.”<sup>2</sup>

There is no question about the general unhealthfulness of this coast at the time Herrera wrote. Malaria, introduced by Spaniards or negroes, would of itself make it so, both to Spaniards and Indians, and could readily account for the difficulty in raising children—more readily than

<sup>1</sup> *Ce*—one; *xiquipilli*—8,000. The word means “an eight thousand”; “a unit of eight thousand.”

<sup>2</sup> The word “*cocoliztle*” is about equivalent to our “epidemic” or, it may be, “epidemic of a dangerous disease.”

yellow fever. Indeed, the heat itself would do that for white children. The custom he ascribes to Montezuma, however, predicates a prevalent, fatal sickness, permanently endemic prior to the Conquest, and fatal to the natives. On what is this statement based? As I have said, I think we now have more, and more trustworthy, data on the Mexico before the Conquest than Herrera could have had, and I find no allusion to any such custom in any record I have been able to see, although many of them, as Torquemada's *Monarchia Indiana*; Sahagún's *Historia general de las cosas de Nueva España*; Motolinia's *Historia de los Indios de la Nueva España*; and Diego Durán's book, are very full of the accounts of native customs. The same is true of some of the Nahuatl *codices* and *anales*. All of these are contemporary, or nearly contemporary, accounts, and their authors would have mentioned this custom had they known of it.

The knowledge of the Spaniards that the coast about Vera Cruz was extremely unhealthful at this time, when Herrera was writing, with no knowledge of its previous sanitary condition, would naturally lead to some such belief as that of the periodic introduction of population from what were then healthful regions, to explain the otherwise inexplicable fact that these lowlands were well populated when first visited. They knew of such movement of population *en masse* as common practice of the Babylonian and Assyrian kings, and it would naturally suggest itself to an ecclesiastical writer as a magnificent exercise of absolute power and of a wise and beneficent paternalism which they were very prone to ascribe to the rulers of the Aztec Empire. On the other hand, Jourdanet, an eminent student of early Mexican history, concludes (in a note to his translation of Sahagún's "*Historia*") that the *tierra caliente* was as well populated before the Conquest and as healthful to the natives as any part of the plateau, and gives reasons for his belief. Similarly, Nicolas de Witte, a friar of the Order of St. Augustine, states that the Province of Huasteca, a Maya settlement on the Panuco river, was densely peopled, "none more so under the sun," when he visited it in 1543, too close to the Conquest to admit of marked increase of population since that event (Terneaux-Compans, 1840, p. 254). There are many other statements which one could quote, showing that the *tierra caliente* was as fully populated at and before the time of the Conquest as the plateau.

Indeed, had the alleged "*cocoliztle*" been yellow fever, and had it been permanently endemic in this region about Vera Cruz, we know that there would have been no depopulation of the locality on this account. The population would be immunized in infancy; and while there would be an



increase of infant mortality, it would not be excessive, and the adult native population would not be affected. From yellow fever there would never have been any need of a periodic repopulation, unless in those parts of a large regional focus through which the infection passed at very long intervals. Malaria will depopulate a community in which it continuously abides—yellow fever never. Moreover, one can scarcely conceive of a more effective method to continue the infection of yellow fever in a region than the method described by Herrera to obviate its ill effects.

It seems, then, to be more than questionable if this account of Herrera is correct. If correct, the “*cocoliztle*,” which this repopulation was supposed to remedy, could not reasonably be considered yellow fever. The writer believes that there were no bases for Herrera’s statements. There is nothing in this account, then, which throws any light on the existence or non-existence of yellow fever on this coast, either before or after the Conquest.

#### REPUTATION FROM OTHER SOURCES

Whether the mortality shown to exist at Vera Cruz soon after the Conquest by the following quotations, given by Finlay (1912, pp. 120–121) from a publication (*Cartas de las Indias* LXXIII) not accessible to the writer, is greater than is reasonably explicable by malaria, dysentery, and tropical conditions generally among newly arrived Europeans, or implies the prevalence of a more deadly disease, the reader must judge.

“Of those who come to San Juan de Lua when, on account of finding no shelter, they are forced to go to the City of Vera Cruz to cure the sicknesses which they have accumulated from Ocoa” (Was this the Ocoa in Honduras?) “to the port, many die because they go to seek health in an unhealthful place and where even those who arrive there sound are accustomed to lose it” (Letter from the Viceroy Enriquez to Charles V, 1572, asking a hospital at Vera Cruz).

Bishop Marroquín of Guatemala, to the Emperor, in 1537: “For the love of God order to provide the same” (a hospital at the port of entry) “in New Spain. There is no year in which 500 men do not die in the port of Vera Cruz, and in the inns and roads a large number.” It is fair to say that the same plea was made by the last petitioner in this same letter for a hospital and medical service at Callao, the port of entry of Peru, where we know there was no yellow fever.

To the writer, sufficient mortality is implied for the sickness to have been yellow fever, but not more than could be reasonably explained

without it, allowing for some exaggeration from the good Bishop and the desire to show the need of a hospital.

#### SICKNESS DURING OCCUPATION BY CORTEZ

This region, Vera Cruz and its environment, was apparently not found unhealthful by the Spaniards who came here with Cortez in 1519, or by the garrison which he left there when he went on to Mexico. Cortez landed there April 22, 1519, on Good Friday, whence its name Vera Cruz, and remained there and at Cempoalla, a large city close by, until August 19. Not only do the historians<sup>3</sup> of the expedition fail to mention any serious sickness among the 550 Spaniards landed there—450 soldiers and about 110 sailors, according to Bernal Diaz—but when many of his men were clamoring to return to Cuba—to prevent which he destroyed his ships—the unhealthfulness of the place was not among the reasons alleged by the mutineers for wishing to leave it. They did allege the long and difficult march necessary to reach the City of Mexico, and especially the report of the power and the organization of the empire Cortez purposed to attack, but not one word about sickness,<sup>4</sup> or that the location was unhealthful. Seeking cause of complaint against remaining, they would surely have alleged this, an entirely sufficient one, had it existed.

A number of minor incidents are related as happening here, among them the death of Cortez' horse and his purchase of another named "El Harriero," a dark chestnut, "from Ortiz the Musician and Bartolomé Garcia, the Miner;" the annoyed embarrassment of Escalante, to whom, as a god, incense was offered when he went to the temple; and the practical joke that Cortez perpetrated on "old Heridia, the Basque," said to be the ugliest among the Spaniards. Our chroniclers seem to have had plenty of both time and space at their disposal, and surely the occurrence of a serious sickness had not failed of mention.

Note, too, that the time, April 22 to August 19, was during the sickly season of Vera Cruz, and that during most of this time the Spaniards were associated with a large native community,<sup>5</sup> in which we would expect to find yellow fever if it were anywhere in this region.

<sup>3</sup> Bernal Diaz del Castillo, and Gomara, who would give Cortez' account.

<sup>4</sup> Except to state that "the few soldiers that we were, and they sick and fed up with marching from place to place": "*tan pocos soldados como Eramos E que Ellos estaban dolientes y hartos de andar de una parte à otra*" (Diaz, *op. cit.*, vol. 1, p. 141).

<sup>5</sup> Cempoalla is given from 20,000 to 30,000 population by Las Casas, and Torquemada agrees.



The only complaint recorded of the locality is that made by Bernal Diaz himself: "It did not happen as he" (Gomara) "said; for what kind of people are we Spaniards not to go forward and to remain in regions in which we do not have profit" (plunder) "and wars" (*op. cit.*, vol. i, p. 166).

Later, after the "*Noche Triste*," July 10, 1520, Caballero, the admiral whom Cortez left in command of the small force at Villa Rica (Vera Cruz), both that ashore and that afloat (in the captured vessels of Narvaez), did report that he had not many mariners fit for duty; that "some were sick and some had died." But as of the seven who accompanied Lencero "five were full of buboes and two were swelled with great bellies," it would seem that this garrison gives no evidence of yellow fever among them. Some of them had been at Vera Cruz over a year, and all a long time.

Cortez' men had come from Cuba, which at that time, and for more than one hundred years later, is believed to have been free from yellow fever (Finlay, 1912). However, a number, possibly a majority, had come via Hispaniola and other Spanish settlements in the Caribbean of which we cannot (at present) so certainly say this; and it is claimed by Finlay (1912) that they had developed yellow fever in these settlements and were therefore immune to further attack. That *all*, including the crews of the vessels destroyed and captured, should have been so immunized, or so many of them that yellow fever in the remainder would not have been noticed, seems to me highly improbable.

The death rate of yellow fever, in the conditions under which these men were living, is high, and even a small number of cases of that disease among them would have caused deaths enough to have been noted. I think we may safely claim that there is no evidence of yellow fever among the Conquistadores at Vera Cruz and that the negative evidence is strong that there was none.

## CHAPTER V

### THE MAYA COUNTRY: CULTURE; RECORDS OF PESTILENCES (MAYA CIMLAL AND XEKIK)

Of the Maya country, it is the lowlands—Yucatan, Campeche, and the low-lying parts of Guatemala—that are in question. Here the temperature is such that *aegypti* would be active at all seasons, and there was a large and dense population—large enough and dense enough, one would think, to provide against the elimination of yellow fever by failure of the human host. The storage of water for domestic use in Yucatan must have been always general. Much of the country is limestone (coral); and streams, except underground, are rare. Their old *chultunes*, we believe, were used for water storage, although it is possible that some of them may have been used for the storing of grain. They would certainly have been ideal breeding places for *aegypti*. Obviously, both physical and sociological conditions were well suited for a *permanent endemic regional focus* of yellow fever. On the other hand, on account of the rarity of water above ground, save in artificial containers, Yucatan should have been to a great extent free from *Anopheles* and hence free from malaria, even after the advent of Spanish and negro carriers. Documents of the sixteenth century (1580 and later) confirm this.

Naturally, in a permanent endemic focus yellow fever would be little apt to be noted in native records, except as epidemics among aggregations of susceptible people coming into the country. Such occasions for epidemics might have occurred when Mexicans from the uplands visited the country, whether for war or commerce, which they did from time to time before the Spanish came,<sup>1</sup> and in a sufficiently developed historical record the epidemics would probably be noted if of great severity.

<sup>1</sup> For more than two centuries before the coming of the Spaniards, Aztec mercenaries are said to have been employed at Chichen Itza and Mayapan (Morley, 1915, pp. 5-6, quoting Landa and others); and, as indicating commercial intercourse, Brinton (1882, p. 23) says that the attack of the Aztecs on the Huastecs in the reign of Montezuma the First was made for the alleged reason that the Huastecs had robbed and killed Aztec merchants on their way to the great fairs in Guatemala.



## EARLY MAYA CULTURE AND RECORDS

The culture of the Mayas prior to the Conquest was easily the highest of any of the American peoples. The Mexicans, as we have seen, had a system of hieroglyphic writing, somewhat conventionalized; and toward the latter part of their empire, when they wished to indicate the names of men or of places (compounded usually of several sounds), these pictorial signs frequently were used to represent these *sounds* instead of ideas. That is, the sign was used for the *name*, or some part of the name, of the object represented, and not for the *object* itself, the writing being then practically what we now call a "rebus." Thus, an ocelot (*tehuan*) on a steep hill or rock (*tepec*), instead of representing what was shown, was read "the Kingdom of Tehuantepec." They were thus on the road, maybe only at the beginning of it, to the use of characters representing sounds only—that is, to a phonetic alphabet.

The Mayas also used a system of ideographs for writing. Their system, however, was much more developed and much more highly conventionalized than that of the Aztecs. Indeed, while almost any one can, to an extent, get the meaning of an Aztec record, few of the original Maya inscriptions have as yet been deciphered, except such as relate to their chronology. The opinion of Mayan scholars is that the use of the Mayan system of signs for writing (Bishop Landa to the contrary, we dare not call them "letters") was well advanced into what we may call the "rebus system of writing," and possibly may have been occasionally used even more phonetically in word building. Judging from the history of our own alphabet, this seems to be one of the stages naturally occurring in the evolution of a truly phonetic alphabet from ideographs.

In this system the Maya records were kept. Of those written in the original hieroglyphics we have numerous inscriptions on monuments, buildings, tablets, and objects of art, and a very few manuscripts. Many more of the latter, some on a sort of paper and some on deerskin parchment, are known to have existed at the time of the Conquest, but have been either lost or destroyed. In the great *auto-da-fé* at Mani, in 1562, Bishop Landa destroyed, among other things, twenty-seven rolls of manuscripts on deerskin (Brinton, 1882, p. 65). The hieroglyphic writings that have been preserved are mostly undeciphered except as they relate to records of time, and it is still a question how much of history they may reveal when more fully deciphered.

In addition to these we have a certain number of manuscripts written after the Conquest, by Maya scholars, in the Maya tongue, but in Roman characters. Like the picture-writings of the Aztecs, these

records claim to give the history of the Mayas from very early times. The earliest date given for an alleged historical event, the date of leaving Tulapan, is *katun 8 Ahau*, of a certain cycle, which Morley (1920, p. 499) places within the 20 years ending 176 A.D. This, however, is not the zero point of Maya chronology, which is extended back to great antiquity. Again like the Nahuatl records, those of the Mayas were kept to some extent during and after the Conquest—one of them certainly to beyond 1648—the disturbing social effect of the Conquest being decidedly less than in Mexico. This was especially the case in districts like those ruled by the Xius and the Pechs, who were consistently friendly to the Spaniards. Ix Nakuk Pech, of the family from which Campeche was named, who, when he “entered the water and received baptism,” was called “Don Pablo Pech” and given the title of “Hidalgo and Conquistador,” continued to rule his father’s province and bequeathed it to his son.

As shown by the records which have been deciphered, the Mayas had an elaborate and exact, but rather complicated, system of chronology. Without going into its complexities, we need to refer to only two units of time used in the documents that we shall quote. These periods, based on the day (*kin*) as a fundamental unit, are: the *tun* of 360 days, and the *katun*, of 20 *tuns* or 7,200 days—nearly 20 (19.713) of our years.<sup>2</sup> The “Sequence of Katuns,” or “small cycle” as we may call it, comprised 13 *katuns*, approximately 256 of our years. In this “cycle” each *katun* was named for the day *Ahau* with which it ended, the ending-day being designated by a number, as 1 *Ahau*, 2 *Ahau*, etc., and the *katuns* known accordingly as *katun 1 Ahau*, *katun 2 Ahau* . . . *katun 13 Ahau*. However, the *katuns*, in their sequence, did not follow each other in straight numerical order, 1, 2, 3 . . . 13; but, owing to the way in which days *Ahau* fell, the order of the *katuns*, beginning, say, (arbitrarily) with *katun 13 Ahau*, was: 13, 11, 9, 7, 5, 3, 1, 12, 10, 8, 6, 4, 2, then returning

<sup>2</sup> In addition to the *kin* (day), *tun* (the primitive meaning of which is stone, rather than year), and *katun*, the Mayas used a *uinal* (month) of 20 days, and a *haab*, or year of 365 days—18 “months” plus five “nameless days”—which had names, indeed, but were counted in no month. The *haab*, however, is not the basis of record on their monuments, the object of such record being, apparently, to mark the number of *days* which had elapsed from their zero day. Their chronology was based on the revolution of the earth on its axis, as ours is on its revolution around the sun.

The cycle which fits into the “great cycle” is of 20 *katuns*. What we refer to as a “cycle” of 13 *katuns* is more properly the “Sequence of the Katuns” or *u kahlay katunob* (Morley, 1915, pp. 37–86).



to 13 and repeating in the same order. Thus, a *katun* of any given number *Ahau* recurred at intervals of (approximately) 256 years.

The recording of time among the Mayas was intimately related to their religious ceremonies and priest-craft—cultivated more for this, perhaps, than for the record of historical events. Thus we find that though they were able to fix time accurately to the very day, they were usually satisfied, in their chronicles, with recording the *katun* in which an event took place. It is rare that even the *tun* in the *katun* is specified; and where this is recorded in more than one chronicle, there are not infrequently discrepancies between them. If these records are a fair sample, the Mayas paid more attention to the record of *time* than to that of events, reversing what we have found to be true of many of the Spanish records of this period.

The Maya records in Roman script that are known are placed by Brinton in two classes: (1) official documents, "titles to the municipal lands, the record of surveys, etc.," and (2) the "Books of Chilan (or Chilam) Balam." "Chilan Balam" was the designation of a class of priests, and the different "Books of Chilan Balam" are distinguished by the names of the villages from which they came, as "The Book of Chilan Balam of Mani," "of Tizimin," etc. Those that are known are devoted partly to the recital of the sequence of *katuns*, with mention of events of history; partly to ritual predictions of future events. These prophecies are especially of calamities which will befall, usually at the "doubling of the *katun*," that is, at the recurrence of a *katun* of the same number *Ahau*, 256 years later.

Brinton (1882), in his *Maya Chronicles*, gives the original Maya text and translations into English of the historical portions of three "Books of Chilan Balam:" those of "Mani," "Tizimin," and "Chumayel." From the last-named he gives two separate chronicles, and a third extract which is not counted as historical, but is a sort of ritual chant.

The manuscripts from which these are taken are said by Brinton (p. 70) to be probably "all copies of older manuscripts, with merely the occasional addition of current items of note." That they actually are copies of written records, seems, however, not to be assured, for in another passage (p. 68) Brinton says of their authors: "Relying on their memories, and no doubt aided by some of the ancient hieroglyphical manuscripts, carefully secreted from the vandalism of the monks, they wrote out what they could recollect of their national literature." Later, commenting on the similarities and discrepancies between the different

chronicles, he says (p. 82): "This similarity may be explained by two suppositions; either they are copies from a common original, or they present the facts they narrate in general formulae which had been widely adopted by the priests for committing to memory their ancient history. The differences which we find in them preclude the former hypothesis except as it may apply to the first two" (the Books of Chilan Balam of Mani and Tizimin). "The similarities in the others I believe are no more than would occur in relating the same incidents which had been learned through fixed forms of narration."

The record of events is scant—the whole of the four chronicles occupy less than 20 octavo pages—with intervals sometimes of more than a hundred years between events noted, and with many irregularities. In such documents we need not expect to find much mention or description of diseases, yet the records do yield some information on our problem.<sup>3</sup>

#### THE MAYA CIMLAL (MAYA-DEATH: PESTILENCE)

Of the four historical manuscripts covering the date *katun* 4 *Ahau* of a certain cycle, given by Brinton, three, the chronicles of Tizimin (*op. cit.*, p. 148), the first of Chumayel (p. 161), and the second of Chumayel (p. 171) agree in the occurrence of what, from its name, must have been

<sup>3</sup> The Maya documents quoted in this chapter are:

1. *The Maya Chronicles*, by Daniel G. Brinton (Phila., 1882). These include, in Maya text, with English translation and notes, the following:

- I. The Series of the Katuns. From the "Book of Chilan Balam of Mani" (pp. 95–135).
- II. The Series of the Katuns. From the "Book of Chilan Balam of Tizimin" (pp. 136–151).
- III. The Record of the Count of the Katuns. From the "Book of Chilan Balam of Chumayel" (pp. 152–164).
- IV. The Maya Katuns. From the "Book of Chilan Balam of Chumayel" (pp. 165–176).
- V. The Chief Katuns. From the "Book of Chilan Balam of Chumayel" (pp. 177–185).
- VI. The Chronicle of Chac Xulub Chen (Chronicle of Chicxulub) by Nakuk Pech, 1562 (pp. 189–259).

2. A photostat, of the entire manuscript of the "Book of Chilan Balam of Chumayel" (107 plates), published by the Museum of the University of Pennsylvania. Excepting the part given by Brinton, there is no published translation of this.

3. Extracts from manuscript copies of the "Book of Chilan Balam of Tizimin" (portions not included in Brinton's *Chronicles*) furnished to the writer in personal communications from Dr. Juan Martinez Hernandez of Merida, Yucatan, and Dr. Herbert J. Spinden of the Peabody Museum, Harvard University.



a pestilence in that *katun*: more definitely placed by one of them (p. 171) as in the fifth *tun* of that *katun*. The end of the previous *katun* is placed by Morley (1920), who has gone critically into the correlation of Christian and Mayan chronology, as 1477, about the end of November. According to this chronology this epidemic should have occurred, then, in 1482 or 1483. According to the chronology of Landa, with whom Martinez Hernandez (personal communication) agrees, it is placed in 1485 (Landa, 1864, pp. 52 and 62).

The original and the literal translation of one of these texts, from the "Book of Chilán Balam of Tizimin"—practically identical with that from the first chronicle of the "Book of Chilán Balam of Chumayel"—is here given (Brinton, 1882, pp. 142 and 148):

"*Can ahau; uchi maya cimlal ocnalkuchil ych paa.*"

"The fourth *ahau*; the pestilence, the general death, took place in the fortress."

The words "*maya cimlal*" (or, as written elsewhere, "*mayacimil*") mean literally "Maya-death" (Brinton, p. 132); and "*ocnalkuchil*," translated above "the general death," means, from its derivation, "buzzards enter the houses," on account of the number of deaths (*op. cit.*, p. 151).

Whether the pestilence was general throughout the land is not indicated unless by implication. The last words seem to limit it to "the fortress." Hitherto in these records this term "*paa*" had only been applied to Mayapan, a large city, the over-lord of the country until it had been "depopulated" (destroyed or abandoned) by a civil war in *katun* 8 *Ahau*, (about 1438–1458 A.D.). Possibly it was not entirely destroyed at that time, or the Maya-death may have been in some other fortress to which the Mayapan people had moved.<sup>4</sup>

The manuscript of Mani, which Brinton thinks (pp. 82 and 136) is closely related in its origin to that of Tizimin, mentions no pestilence in this *katun*, but gives:

"*buluc ahau uchci mayacimil ich paa yetel nohkakil.*"

"The eleventh *ahau* took place the pestilence in the fortress and the smallpox" (Brinton, 1882, pp. 98 and 103).

<sup>4</sup> Dr. Martinez Hernandez of Merida says (personal communication) that this term "*ich paa*," translated by Brinton as "in the fortress," is "the name given to the population of Mayapan living *outside* the stone wall surrounding the capital of the Maya Confederation." "Izamal and Mani, founded after the destruction of Mayapan, had no walls." "*Ich paa* and Mayapan are synonymous." "The pestilence refers to an existing condition in Mayapan." This would limit the pestilence to what was left of Mayapan.

Here the "pestilence in the fortress" is placed between the years 1536 and 1556, that is, during the period of the Spanish Conquest, and 40 to 80 years later than the date given by the other three records.<sup>5</sup> Here too, smallpox,<sup>6</sup> though seemingly differentiated from "*mayacimil*" by the "*yetel*" ("and"), is associated with it in time.

There are similar discrepancies between the different chronicles in their records of smallpox—the only disease other than "*mayacimil*," "*ocnalkuchil*," that they mention.

The manuscript of Mani, as we have seen, notes smallpox first in *katun* 11 *Ahau* (1536–1556), and later records it in 1609, giving the year A. D. The Tizimin also mentions smallpox twice: first in *katun* 2 *Ahau* (1497–1517) and again in 1569. The latter entry is evidently meant to correspond to the 1609 given by the Mani manuscript, for in all other dates given in Christian chronology, from 1536 to 1611, these two documents agree. The first of the Chronicles of Chumayel agrees with the Tizimin in mentioning smallpox ("*kakil nohkakile*") first in *katun* 2 *Ahau* (1497–1517), and says, in the record of *katun* 11 *Ahau* (1536–1556): "mighty men came from the East" (Spaniards) "they brought the sickness." Brinton (p. 164) thinks this was probably smallpox,

<sup>5</sup> According to the correlation of Maya and Christian chronology given by Morley (1920, p. 499), the sequence of the *katuns* and the years of our calendar corresponding to each were as shown below for the period from the destruction of Mayapan to 1615. Each *katun* (of 19.713 years) includes a part of the calendar year in which it begins and of that in which it ends.

		Year A.D.	
		<i>began</i>	<i>ended</i>
katun 8	Ahau.....	1438	1458
katun 6	Ahau.....	1458	1477
katun 4	Ahau.....	1477	1497
katun 2	Ahau.....	1497	1517
katun 13	Ahau.....	1517	1536
katun 11	Ahau.....	1536	1556
katun 9	Ahau.....	1556	1576
katun 7	Ahau.....	1576	1596
katun 5	Ahau.....	1596	1615

<sup>6</sup> The (partially) descriptive names under which smallpox appears in these records are: "*kakil*" (fire: burning), "*nohkakil*" (great fire), and later ("Book of Chilan Balam of Chumayel," Gordon's photostat, pl. 104) we find it as "*pom kakil*"—gum copal (incense) fire—more descriptive yet, as burns from the sputtering sparks of burning gum copal incense.



though without its distinctive name. If so, this agrees with the record which the Mani chronicle gives of smallpox in this *katun*. The fourth of our sources, the second of the Chronicles from Chumayel, does not mention smallpox at all. We may note, too, that of the three which do mention it, not one of them agrees with the record from European sources, according to which smallpox was introduced into Cozumel in 1520 by Narvaez.<sup>7</sup>

In the only passage where smallpox and "*mayacimil*" are mentioned together, that is, in *katun* 11 *Ahau* (1536–1556), by the Mani manuscript (Brinton, pp. 98 and 103), they seem to be differentiated by the text: "*mayacimil* in the fortress and ('*yetel*') smallpox." Yet one wonders whether, in spite of the "and," the "*mayacimil*" of this *katun* were not itself smallpox. It may even be a question whether the "*mayacimil*" of *katun* 4 *Ahau* may not have been smallpox, misplaced in the time of its occurrence. That events prior to the Spanish Conquest were rather frequently misplaced in these records is admitted. However, we think the evidence here is decidedly in favor of a pestilence ("*mayacimil*") other than smallpox having actually occurred in *katun* 4 *Ahau*, that is, between 1477 and 1500 A.D., taking Morley's chronology for the beginning and Landa's for the ending of this *katun*. We have, indeed, independent evidence of this from European record of Maya tradition current in the time of the Conquest.

<sup>7</sup> The writer has noted two systems connecting the Mayan and Christian chronologies: that by Bishop Landa (1864) and that given by Morley (1915 and 1920). These, about this period, differ by about two and a quarter years. Thus, the date in our calendar corresponding to the 5th *tun* of *katun* 4 *Ahau* of a certain cycle is given by Morley as 1482 or 1483; from Landa it would be 1485.

As Morley has the information Landa gave for his own determination, and in addition all that has become available since (and much work has been done in Maya chronology in the last 100 years), his determination should be the more accurate; and from examination of his data and arguments, the writer so regards it. Yet the first record, in the manuscripts of Tizimin and of Chumayel, of the appearance of smallpox among the Mayas is given as in *katun* 2 *Ahau*. This ends, according to Morley, in 1517: according to Landa, in 1520. The latter would agree with its introduction in 1520 by Narvaez at Cozumel.

Of course we cannot be absolutely positive that this was its first introduction, but we have no *European* evidence of smallpox in either Yucatan, Mexico, or Central America before that date or—unless we take these passages with Morley's chronology as being such—in any Maya or Nahuatl records that I have seen, and it seems unlikely, had such a thing occurred, that it had not been noted. Also, the Cozumel epidemic of 1520 was evidently in a virgin population in that section.

The foregoing is evidently, then, evidence, as far as it goes, in favor of Landa's chronology as compared with Morley's.

Of the nature of the sickness called "*mayacimil*," the writer can find no description whatever in these chronicles or in any other historical document of the Mayas<sup>8</sup>—not even such descriptive name as we had in the word "*matlazahuatl*" for the Mexican pestilences of 1544 and 1576. Such Maya scholars as I have interrogated personally or by letter believe that no such description exists in the Maya records. It is simply "*maya cimlal*" ("Maya-death"), a deadly pestilence; the name showing the gravity of the sickness, and "*ocnalkuchil*" ("buzzards-in-the-houses") to express that it was a great epidemic independently of the nature of the sickness—about equivalent apparently to the word "*cocoliztle*" in Nahuatl.

We have, indeed, from two Europeans, quoting Maya tradition, accounts of great sicknesses occurring before the Conquest. One of these is by Bishop Landa, written probably about 1565 (Morley, 1915, p. 7); the other is by Benzoni, quoting a Maya *cacique* about 1541 or 1542. Both of these accounts were written close enough to the time of occurrence (about 1485) of the "*maya cimlal*" to be of value.

However, we can better discuss these later; and leaving them for the present, let us first examine the passages in the Maya manuscripts where the word "*xekik*" (blood-vomit) occurs.

#### REFERENCES IN THE MAYA RECORDS TO XEKIK (BLOOD-VOMIT)

In two of the Maya manuscripts that have been quoted, the "Book of Chilan Balam of Tizimin" and the "Book of Chilan Balam of Chumayel"—but in portions not included in Brinton's *Maya Chronicles*—there are passages containing the word "*xekik*," which, from its meaning (blood-vomit) and the context in which it occurs, is evidently the designation of a fatal epidemic disease characterized by the vomiting of blood.

So far as the writer knows, the first to call attention to these passages and to relate them to the history of yellow fever was Bishop Crescencio Carillo y Ancona of Merida, a distinguished Yucatecan scholar who, at

<sup>8</sup> The Chronicle of Chac Xulub Chen (Brinton, 1882, pp. 189–259), written about 1562 by Nakuk Pech, the hereditary ruler of this district, is historical for the time it covers, and is probably the most reliable record that we have. It is a contemporary account by an intelligent and educated man of high position before and after the Conquest. It is especially valuable for its chronological data, stating that a *katun* ended in the year 1517, and giving other dates which Morley (1920, p. 468 *et seq.*) considers important for the correlation of Maya with Christian chronology. The document covers, however, only from 1511 to about 1562, the early part being from the reports of others. There is no mention of the "*maya cimlal*" or of smallpox in it.





Timiran Caan y luum ti bay Janil tu Diosil  
 tumuyalit. tu batukunal Cau cih cah bal cah  
 bicinil. Capecnahi tucaanil tukuil ti bay non  
 u Chucil yanil ah tepale  
 Vholan kin Gan camal licil vxocol.  
 Vchan tilikine habix holanile

layukabab Vici Julo bla

Demilein nienton idies ymucbe anor. hylae

+ 1519. Layukabil yan cau li Julo b. Vay  
 hac cahat coon ah yha Vytli luum yu  
 cal peten yucatan tu than maya ah yha  
 ah lae.

Bay Jalci yax a Delan tado. D<sup>n</sup> Juan de  
 Montojo: yoklal bay alab ci ti tumen  
 D<sup>n</sup> Lorenzo Chable = vyube lay con cix  
 vidon tix Ho Kobe: ti tumen du lob auu  
 auu ci ti: tu chon u katabi D<sup>n</sup> Lorenzo  
 Chable: yoklal u ual kati til baki. Vhan  
 te du lob: y Capitanob tula cal: yanix v Mahen  
 D<sup>n</sup> Martin Chable u kaba xan

+ 1540. Layukabil Juximbal. Cahapi u chancubavulob  
 Viatu enucicob Vay Jucal peten lae =  
 vyohel cahix ah kin: Ah bouat: ah xupan  
 v kaba = Oc ci Christiano il coon = D. 1519 =  
 Q. Lahci kina ti Doo: D. 1540 = coo  
 ci kina ti Doo: D. 1599 = Vch cix e  
 ti: kop ci cimat non D. 1648 anor

FIG. 4. PAGE FROM THE BOOK OF CHILAM BALAM OF CHUMAYEL, SHOWING (IN LAST TWO LINES) REFERENCE TO THE OCCURRENCE OF "xekik" IN 1648

Reproduced from Gordon's photostat of the original manuscript (Anthropological Publications of the University of Pennsylvania, vol. 5, Philadelphia, 1913, plate 63).



the time, possessed the original Tizimin and Chumayel manuscripts. Finlay, noting the reference to "*xekik*" in one of the Bishop's publications, wrote to him and received in reply a letter giving further information and an additional citation from the manuscript. This letter is quoted at length by Finlay (1912, pp. 211-219) in his paper,<sup>9</sup> which interprets the passages as giving strong evidence that the Mayas had ancient knowledge of yellow fever.

We will quote the original Maya text and translations of four passages from the manuscripts of Chumayel and Tizimin, including the two which Finlay gives in his paper.

### Quotation I

The "Book of Chilam Balam of Chumayel" (Gordon's photostat, Philadelphia, 1913, plate 63) gives:

"*uchci xekik hoppci cimil toon dzo 1648 años.*"

Translation: "Occurred blood-vomit began death to our people the year 1648."

This, which is the first passage quoted by Finlay, is obviously an historical record, and refers to a time more than a hundred years after the Conquest. It is quoted here as showing: (1) that this epidemic was recorded when it occurred, and (2) that "*xekik*" was the name the Maya used to describe the sickness which we are sure, in this instance, was yellow fever—the epidemic of 1648 being historical.<sup>10</sup>

<sup>9</sup> This paper, entitled "Yellow Fever, Before and After the Discovery of America," is included in Finlay's *Trabajos Selectos* (1912) as a reprint from *The Climatologist*, Philadelphia, July, 1892.

<sup>10</sup> Of course "black vomit," which gave the Spanish name to yellow fever and which is its most readily noted characteristic, is the vomiting of blood. Its appearance, however, save in the (usually rare) fulminant cases, does not suggest blood and, in fact, was not recognized as such by European physicians until 1738, by Henry Warren. Nevertheless, in the great early epidemics which occurred in the Americas (1648 in Yucatan and the French Antilles; 1686 in Bahia), "the bringing up of blood"—"the discharge of blood from the mouth"—was recognized by those who described it. I think, then, that "blood-vomit" would have been naturally used by the Mayas as a designation of a *severe* epidemic of yellow fever.

Bishop Carillo y Ancona of Yucatan, in his letter to Dr. Finlay (Finlay, 1912, p. 216), quotes what we have just given and makes an argument for knowledge of yellow fever by the Mayas *prior to 1648* because they *at once* gave a name ("*xekik*") to it in their records for this year. The same argument would hold for smallpox, which was, however, recorded as "*nohkakil*" ("great-fire") when it first appeared among them in 1520. A name, "*tohtomonalitzli*"—to have pustules—was also given to smallpox by the Mexicans when it first appeared among them. This name occurs in the entry for the year 1520.

*Quotation II*

To Dr. Juan Martinez Hernandez,<sup>11</sup> of Merida, Yucatan, (personal communication of July 13, 1922) the writer is indebted for the following extract from the "Book of Chilan Balam of Tizimin" (MS. folio 15, p. 29):

*"xekik, u cuch katun te tu uich can ahau katun vale, uchci maya-cimlal ocnal-kuchil, bay bin uchpahal tu vuudz tu caten, ti cu tal tu cuch-habil, bay yalci ahkin chilan tu cuch-katun can ahau vale."*

This is translated by Dr. Martinez Hernandez as follows:

*"'blood-vomit is the outcome' (burden) 'of the katun, in the transcourse of the katun four Ahau, probably, took place' (the calamity or pestilence called) 'maya cimlal' (the pestilence of the Mayas) 'and ocnal-kuchil' (when so many died that the buzzards entered the houses to eat the corpses: such statement was made by the High Priest); 'so it will again take place, when it turns again, it is indicated in the course of the yearly events, as was predicted by the High Priest, as the coming events in the course of the katun four Ahau, probably.'"*

This is clearly a prediction, rather than an historical record. The expression "when it turns again (*vuudz*)" refers to the doubling of the *katuns*, in this case when *katun 4 Ahau* returns in the count of the *katuns*. The words "so it will again take place when it turns again (*bay bin uchpahal tu vuudz*)" are especially to be noted for comparison with phrases which occur in Quotations III and IV. This quotation predicts a return of the "*maya-cimlal*" and "*ocnal-kuchil*"—that is, "Maya-death," "great epidemic"—during the next *katun 4 Ahau*, which should run from about 1733 to 1753, and that *xekik* will occur at the same time. The direct statement as to the past is that "*maya-cimlal*" and "*ocnal-kuchil*" occurred (*uchci*) in *katun 4 Ahau*. This does not seem to be directly stated of "*xekik*," though such may be the intent of the phrase "*xekik u cuch katun*," which Dr. Martinez Hernandez translates literally "blood-vomit is the burden of the katun."

*Quotation III*

The next passage is one of those quoted by Finlay, who received it from Bishop Carillo y Ancona, the source being the Tizimin manuscript (folio 16 (*verso*) and 17). Transcripts of the same passage from this manuscript have also been furnished the writer by Dr. Martinez Hernandez and Dr. Herbert J. Spinden of The Peabody Museum of Harvard University.

<sup>11</sup> I can scarcely adequately express my gratitude to this gentleman for his many courtesies and for the assistance he has given me in this and other matters.



The passage is:

"*Can ahau u buluc dzit katun cu xocol tu chichen ytza u hedz katun ULOM kuk ULOM yaxum* (p. 33) *ULOM ah kantenal ULOM xekik tu can UADZ ULOM kukulcan tu pach ah ytzaob, tu CAN TEN u than katun vale.*"

For this we will give three translations. The first is by Bishop Carillo y Ancona. The English equivalent of his Spanish translation is given by Finlay (p. 217) as follows:

"In the fourth ahau (year of the Mayas) at the expiration of a *katun* (*their century*) which is counted towards the well of Chichenitzá, at the placing of the stone katun, arrival of Kuk, arrival of Yaxum (*mythological and historical characters who gave their names to the epochs*)<sup>12</sup> arrival of Kantenal, OCCURRED<sup>13</sup> THE ARRIVAL OF BLACK VOMIT FOR THE FOURTH TIME, arrival of Kukulcan, after the Ytzaes, at the fourth placing and signification of the katun."

Note that the word "*ulom*" preceding "*kuk*," "*yaxum*," "*ah kantenal*" and "*kukulcan*" is translated "arrival of" (*llegada de*), with no indication of tense, whether past or future; while, as referring to "*xekik*," it is translated "occurred the arrival of" (*fué llegada de*) "*xekik*." This must be considered an "interpretation" rather than a literal translation, for the word "occurred" (*uchci*) is not in the Maya text.

The second translation is by Dr. J. Martinez Hernandez (personal communications of April 6, 1922, July 13, 1922, and July 3, 1923), who renders the Maya text, *as written*, thus:

"Four *ahau* the eleventh *katun* reckoned at Chichen Ytza, it establishes the *katun*, will arrive *kuk*, will arrive *yaxum*, will arrive *ah kantenal*, will arrive *xekik* for the fourth time, will arrive *kukulcan* with the Ytzas the fourth time according to the *katun* probably."

The material difference between this and the first version is that the word "*ulom*" translated by Bishop Carillo y Ancona as "arrival" and given the sense of "did arrive," is translated by Dr. Martinez Hernandez as "will arrive."

Of the Maya text itself, which he considers "a brief ritualistic recital" of Quotation II, he writes: "I am afraid that the word '*uadz*' (the fourth

<sup>12</sup> An examination of Brinton's *Names of the Gods in the Kiche Myths of Central America* (1881), and of his *Nagualism* (1894), gives none of the above names except "Kukulcan." Recent Maya scholars state that in the early attempts to translate Maya records it was common to consider nouns of which the translator did not know the meaning as names of their gods or legendary heroes.

<sup>13</sup> This word "occurred" ("*uchci*") is not in the Maya text of which this is a translation.

time) should be '*uudz (vuudz)*' as in the first instance" (Quotation II) "no such fourth time of *xekik* is mentioned, but '*uudz*,' the doubling or shifting movement of the *katun*."

"*Caten* (again) is written twice" (Quotations II and IV); "it should not be read (here) *can ten* (the fourth time), but *caten*, twice; again;" and he translates Quotation III, thus changed, as follows:

"Four *ahau*, the eleventh *katun* reckoned at Chichen Itza, it establishes the *katun*; will arrive *kuk*, will arrive *yaxum*, will arrive *ah kantenal*, will arrive *xekik*, when it turns again will arrive *Kukulcan* with the Itzas again according to the *katun* (ritual) probably."

The writer, of course, cannot claim to be able to decide between Maya scholars, but as authority for his translation of "*ulom*" as "will come," Dr. Martinez Hernandez cites Father Gabriel de San Buenaventura, who should be authoritative on the old Maya language, having published his grammar of it in 1684, in Mexico. He is very clear as to the signification of "*om*" appended to a verbal root—making what he called "the future of certainty." He says:

"*Om*: with this are made the futures in *om*, which come only from verbs of the first conjugation, changing the ultimate '*i*' of the third person of the preterite into '*om*:' e.g. *cimi*—he died: *cimon*—we will die without fail. These futures mean that the action which the verb signifies has to happen without fail."<sup>14</sup>

Dr. Martinez Hernandez writes that this use of "*om*" is antiquated, and is given only by Buenaventura; also that "*ulom*," he thinks, does not here express a future of *certainty*, but is better translated "is to arrive."

The sense in which Bishop Carillo y Ancona has interpreted "*om*" is that it indicates "us," "to us," from which "*ulom*" would be "arrival to us," and this, as we understand from Dr. Martinez Hernandez, would be the usual rendering, the use of the ending "*om*" to indicate the future being, as he has said, antiquated.

Even independently of the question of grammar involved in the rendering of "*ulom*," reference to the future seems clearly implied (1) by the prophetic character of the whole passage and its relation to the prophecy

<sup>14</sup> "Om. Con esta se hazen los futuros en Om, que salen de verbos de la primera conjugacion sola, mudando la I. vltima de la tercera persona del preterito en Om, vg. Cimi murió: Cimon, moriremos sin falta. Significan estos futuros, aver de suceder sin falta, lo que el verbo significare; v.g. Mail Manom chibal lae, *non praeteribit generatio haec*: He inthane mamanoml, *Uerba autem mea non praeteribunt. Uide futuros en Om.*" San Buenaventura, Gabriel de, *Arte de la Lengva Maya*, 1684, l. 33 vo.



in Quotation II; and (2) by the expression "*ulom kukulcan*." Kukulcan, analagous to the Quetzalcoatl of the Mexicans (Brinton, 1882, p. 119), was the legendary hero-god of the Mayas, bearing some such relation to them as Arthur to the Britons or Holgar Danske to the Danes; and his *second coming* was looked forward to as the deliverance from their oppressors. The arrival of Kukulcan was evidently not meant as a calamity, but was prophesied as a deliverance after a culmination of calamities. It was as evidently to be in the future.

Of more importance for our purposes, is the question raised by Dr. Martinez Hernandez whether the words "*uadz*" and "*can ten*," signifying "the fourth time" are not due to errors in copying the words "*uudz*" and "*caten*," meaning "twice." Both Finlay and Bishop Carillo y Ancona, as quoted by him, interpreted this passage as referring to the epidemic of 1648, which is well authenticated as yellow fever, and based their argument for *previous* knowledge of this disease on the designation of this as its *fourth* occurrence.<sup>15</sup> That errors in the copying of these manuscripts were very frequent is noted repeatedly by Brinton, and is indeed obvious enough in the recording of proper names and dates. It is, then, by no means improbable that such errors should have occurred in this text. Also, the corrections suggested by Dr. Martinez Hernandez make a more rational version—if we can judge of what is "rational" in documents of this character. It seems strange, too, that the Mayas should record the *fourth* occurrence of a particular kind of pestilence when we have no record of its first, second, and third occurrence. The writer is inclined to believe that the words signifying "the fourth time" are errors. At best they make a doubtful basis for accepting three previous occurrences of "*xekik*."

#### Quotation IV

In the "Book of Chilan Balam of Chumayel" (Gordon's photostat, plate 99), we find:

"1740. *Primero*

*Can ahau katun u buluc dzit katun cu xocol chichen ytza: u hedz katun ulom u cahal ah ytzaobi: hulom kuk ulom yaxum: ulom ah Kantenal: ulom xekik: hulom kukulcan: tu pachob tu cate u than D<sup>s</sup>.—lae—ulom ah ytza."*

Dr. Martinez Hernandez translates this as follows:

"Four *ahau*, the eleventh *katun* reckoned at Chichen Ytza, it estab-

<sup>15</sup> Both of them believed, however, that this was its *first* occurrence since the Spanish invasion.

lishes the *katun*, will arrive the men of Ytza, will arrive *kuk*, will arrive *yaxum*, will arrive *ah kantenal*, will arrive *xekik*, will arrive *Kukulcan* with them for the second time, by the order of God, will arrive the Itzas."

This is so nearly identical with Quotation III that, as Dr. Martinez Hernandez says, "it seems to be a rough copy of it," or perhaps, as one might surmise, another version of a formula committed to memory. Note that it is dated 1740, and that the Spanish word D<sup>s</sup>. (Dios) occurs in the text.

On plate 103 of the Chumayel manuscript (Gordon's photostat) there is again mention of "*xekik*." This, with its continuance on plate 104, is so manifestly a prophecy—the high priests are mentioned by name, with the word "*profeciado*" attached to each name and to the word "*sacerdote*"—and is so manifestly written after the Conquest—Latin and Spanish words, some with Maya terminations and some not, being interpolated in the body of the text, and smallpox mentioned as among the calamities that were to occur (this time in *katun 3 Ahau* and *katun 1 Ahau*)—that discussion of it seems unnecessary. Here the word for smallpox is "*pomkakil*," i.e. "gum-copal-fire," or "incense fire" (gum copal being used for incense in the Maya ritual), a descriptive designation from the kind of burns the sputtering gum would make.

Prophecies though they be, these entries, as we have them, indicate that their authors undoubtedly had knowledge of some pestilence characterized by what they considered vomiting—or it may be the spitting—of blood. If either the original or its transcription was written *after* the epidemic of yellow fever of 1648, it would be natural that *xekik* should be given as among the calamities, or as *the* calamity predicted, being the gravest the transcribers had experienced. Even if written or copied after the great Mexican epidemics of 1544, 1545, and 1576, so marked by vomiting of blood, the use of this word as a designation of a pestilence predicted would not be surprising, and this even if no disease with this symptom had hitherto occurred among the Mayas. The occurrences and characteristics of the Mexican epidemic would have been known to educated men in Yucatan, and well might have impressed them profoundly as the type of a pestilential calamity. If, however, the paragraph was in its present form *prior to 1544*, I think it would be very strong evidence that a disease known as "*xekik*" (blood-vomit) was known to the Mayas in Yucatan before the advent of the Spaniards.

Now for the dates at which the copies of the records that we have were made. In the Chumayel manuscript, from which our Quotations I and IV are taken, the entries are *dated* 1648 and 1740, respectively, so



there can be no question that they were written after the historical yellow fever epidemic of 1648. According to Brinton, this manuscript is a copy made in 1782 by an Indian, Don Juan Josef Hoil. Plate 81 of Gordon's photostat shows his signature, "D<sup>n</sup> Juan Josef Hoil" and the date, January 20, 1782.

The Tizimin manuscript is believed to be of earlier date. Bishop Carillo y Ancona, who owned the original, says in the letter quoted by Finlay (1912, p. 219): "Although the quotation from the 'Tzimin' manuscript does not contain the date (expressed in years of the Christian era) it fortunately happens that the book commences its chronological notes with the year '1593', so written in the hand of the Indian author and with the same figures that we now use (folio 1, line 2)." That it was either transcribed or added to later is shown by the fact that the last entries in the part translated by Brinton (1882, pp. 143 and 149) are 1610<sup>16</sup> and 1611. It may, of course, date from a considerably later period, and it was evidently the opinion of Bishop Carillo y Ancona that it extended beyond 1648, for, as quoted by Finlay (p. 219), he says: "to this same year" (1648) "must correspond that *fourth epidemic* mentioned in the 'Tzimin' manuscript, and the three previous ones must have taken place before the year 1593, at which that chronological record begins." The argument which he makes for ancient knowledge of "*xekik*" among the Mayas is based not on the antiquity of the document, but on its allusion to the recurrence of "*xekik*" for the *fourth time*.

It seems, then, that whatever may have been the date of the earliest occurrence of "*xekik*," the Maya manuscripts in which we find mention of it were written (or copied) quite certainly after the Mexican epidemics of 1544 and 1576, which were characterized by the vomiting of blood. Moreover, at least two of the passages—those from the manuscript of Chumayel—are shown to have been, and the other two, so far as we know *may* have been, written *after* the epidemic of 1648 in Yucatan. Hence, the mere fact that "*xekik*" is mentioned in these manuscripts does not, of itself, afford certain evidence that the disease so designated was actually known to the Mayas before 1648.

The three passages which are clearly ritual prophecies (our Quotations II, III, and IV) all refer to a *katun 4 Ahau*, implying that this *katun* is in some way identified with "*xekik*," either as the time when it is alleged to have occurred or when its recurrence is predicted, or both. It is also in a *katun 4 Ahau*, apparently the one extending from 1477 to 1497, that

<sup>16</sup> The date printed as 1610 in the translation (p. 149) is printed as 1619 in the Maya text (p. 143).

three of the four "historical" Chronicles given by Brinton record the occurrence of the pestilence—"mayacimil," "ocnalkuchil." May we conclude from this that the "mayacimil" before the Conquest was itself "xekik"? Certainly this is suggested, but it does not seem especially probable. In the records which purport to be historical for the period before the Conquest, we have only the non-descriptive terms "mayacimil" and "ocnalkuchil." If this served to mark *katun 4 Ahau* as a period of pestilence, it would seem natural that after "xekik" became known it might have been identified, in the minds of the natives, with the then traditional pestilence of the past, itself already associated with their *katun 4 Ahau*, and thus might have come into their predictions.

In addition to the various books of Chilan Balam, from which these extracts are taken, there are a number of what may be called "prescriptions" for "xekik" in Maya manuscripts—in "The Book of the Jew," for example. We seem to have no data as to the dates of these: whether they preceded the Conquest or not. The copies we have were transcribed long afterwards. Until these data are forthcoming these "prescriptions" throw no light on our subject. It is interesting to note, however, that they do show that, in this matter, Mayan psychology was identical with that of other unrelated primitive people. Just as the Romans regarded even a sight of the yellow bird, *Icterus*, a cure for jaundice, and as the English hung red curtains about the beds of those sick of scarlet fever or smallpox, so the Mayas regarded red feathers as the proper remedy for blood-vomit.

#### DESCRIPTIONS OF MAYA PESTILENCES BY EUROPEANS FROM MAYA TRADITION

(1) The description which Bishop Landa (1864, p. 60), from Maya tradition, gives of the "*maya cimlal*" is as follows: "Then began over all the land some pestilential fevers which lasted twenty-four hours, and after that they ceased they" (the people) "swelled and burst, full of worms, and of this pestilence many people died." Landa came to Yucatan in 1549 and finished his book between 1562 and 1566. In getting the data for it he had unusual facilities, including the assistance of Don Juan Cocom of the family of Cocomes, rulers of Mayapan, "a man of high standing and learned in the history of his people." Landa's arrival was, then, between sixty and seventy years after the reputed date of the pestilence (1483 or 1485). Yet, even with a greater interval of time, one would have thought that if vomiting of blood had been so



marked a symptom of this pestilence as to give its name to it, this would have been remembered and the name mentioned.<sup>17</sup>

(2) Benzoni (1565, p. 98), arriving in Yucatan about 1541, quotes a *cacique* "of more than 110 years" as telling Montejo: "In the time of my youth there came in this country a general malady so strange and so cruel that men vomited worms by the mouth, of which there was a *peste* and a mortality so great that we thought we would all die thereof." This was between fifty-four and fifty-seven years after the epidemic, and an old man, even if not "of more than 110 years" as Benzoni claimed, should have remembered so marked a symptom as the vomiting of blood had it been a characteristic of the disease. This indeed, seems rather to connect up with the picture in the *Codex Telleriano-Remensis* of the "mortality" of the year 1 Rabbit (1454) which, considering the locality in which it occurred, could not have been yellow fever.

(3) The "*pestilencia de unos granos*" (the plague of pustules or bumps), which Landa records as occurring thirty-six years after that of "worms and swellings," from its description should have been smallpox, and this coincides fairly well with the introduction of smallpox in 1520 at Cozumel. Thirty-six years after 1485, the date he would give to the occurrence of the "*maya cimlal*," would give 1521; and as the Maya year does not begin on January 1, one year of their calendar would necessarily cover parts of two consecutive years of ours, and vice versa.

Neither Landa's "plague of the swellings" nor the one Benzoni's *cacique* had in mind, whether the "*maya cimlal*" or not, can well be accepted as "*xekik*," a sickness marked by the vomiting of blood, or as yellow fever. Note that the "plague of swellings" seems to have occurred about the date (1485 A.D.) ascribed to the "*maya cimlal*" in three of the Maya historical records.

Finally, the writer must confess that he has been unable to adduce

<sup>17</sup> Take this account of Landa's—clearly from Maya tradition—in connection with the "occurrence of *kuk*" in Quotations III and IV. This word means "a sprout: a new growth." Dr. Spinden (personal letter, 1922) gives "*kukul*" as "a hard conical swelling," as does the Dictionary of Pio Perez (1866-77, p. 186). Landa's informant then may have had in mind buboes which swelled and broke and became infested with maggots. Labat (1722, p. 73) notes: "*des buboes—aux aisnes, les unes plein de sang caillé noir & putant, & les autres plein des vers*," as one of the manifestations of the epidemic at Martinique, as do some of the historians of the disease (diseases?) which prevailed at Isabela soon after its foundation. Buboes from venereal disease would scarcely be epidemic, and yet a number of them, infested with maggots—"plein des vers," as Labat records—might well impress a Mayan observer, as they evidently did the Europeans.

any valid evidence for the existence of yellow fever among the Mayas prior to 1648 from such of their records as are available to him. Naturally, he can not tell what future research in the Maya territory may bring to light. Yet if the Maya records show nothing that we can accept as evidence for the existence of yellow fever in their country before the Conquest, they show nothing for this period that excludes its presence, for had it existed as an endemic disease it had been little apt to be noted in their records.

However, if the native records by themselves give no evidence that is conclusive, either positively or negatively, an inquiry into the sanitary state of this region during and soon after the Spanish Conquest should throw some light upon its previous sanitary condition. It should assuredly do so if it had been a permanent endemic focus of yellow fever, and this inquiry we shall now take up.

EDITORS' NOTE: Since the author's death Dr. Sylvanus G. Morley, in a personal communication of January 27, 1929, has furnished us the following additional extracts from the "Book of Chilán Balam of Tizimin." The extracts were made from the original manuscript by Dr. Ermilo Solís Alcala, who gave them to Dr. Morley with his translations into Spanish. The translations from Spanish into English are by Dr. Morley.

It will be noted that the second of these extracts is identical, in the Maya text, with that given by Dr. Carter as his "Quotation III." That it is referred to a different folio is, we presume, due to the use of a different manuscript copy. It will be noted also that the first and third extracts given here are very similar, suggesting that the latter is a condensed version of the first.

Dr. Morley states that he "believes the *katun* 4 *Ahau* mentioned in these pages must have been that which began in 1477 and ended in 1497." At the same time he calls attention to the fact that the text refers to the horse, which came to Yucatan only with the Europeans.

1. *MS. de Tizimin, folio 17, vuelta:*

"Uucí yabnal u hodz katún ti can ahau, uucten u yail tu Chicheen, macchahom u uich ti nohol, yahez u ne zasztamba, ah bal cab cohom, macan u uich, cimen u uich, yok u uah, yokol yaal, lie u tzicilob u than, u kat tzicil, ma yolah tzic, bay u bel yahaulil cabob; coc ach tu uah, tu yaal; bin dzococti tu tzicilil, hun pay yol, matan u tzic, halici tu cal tzicil tu hunale; chikintanhan u pop, u dzam; maya cimil u cuch, ti tali zatay batabili, muchlahom u halach uinicilob cahil, xekik u cuch katun, tu kin yan hom a uex, zac a nok, ixchanben uah u uah, ulom kuk, ulom yaxum tu kab kaxte, ulom kayum mut, ulom ah tzimin tepoy, ulom ah tzimin mucuc patan tu Chicheen; tan coch katun utzi, tan coch ixma utzi; lay bin tepalnac ah chicum ek tu canil; yaxal am, yaxal cocay; mute ahahzah, maix mac ahom tumen mute chenchen u xicin ti babal tulacal. Te tu uich can ahau katún uale uchei maya cimil ocnal kuchil; bay bin uch pahbal tu uudz tu caton ti cutal tu cuch haabil; bay yalci ahkin Chilam tu cuch katún can ahau uale."



1. *Translation:*

Uuciyabnal erected the *katun* (stone) in (*katun*) 4 *Ahau*. Chicheen is very unfortunate: the *milpero* (corn-grower) at rising, turns his face toward the south, with his face covered up, with his eyes closed, lamenting his food; when they salute him, he does not wish to answer, as he does the governing of the lands. He is very niggardly in the food he eats, they stop saluting him, the others, because he does not salute them, he is proud; his mat and his bed face toward the west; mortal epidemics, foreign lords, governors reunited. Vomit of blood is the plague of this *katun*; with trousers torn behind, with white clothes they look for bread; there appear the nests and the green shoots in the branches of the trees; there comes the pheasant which sings; there arrives to us the horse with the money-bag on the harness, with the money-bag for the tribute at Chicheen. Half of the *katun* is good, half is bad, as they are good or bad the stars reign in the sky. Green spiders, green glow-worms, the pheasant which wakes, he who is not awakened by the pheasant it is because he is completely deaf. During (*katun*) 4 *Ahau* there was an epidemic so mortal that the crows entered the houses. Thus it will happen when there comes back to begin again another *Ahau* (*katun*) like this one. Thus said the priest Chilan about the next *katun* 4 *Ahau*.

2. *MS. de Tizimin, folio 20, vuelta, tercer párrafo:*

“Can ahau, u buluc dzit katún cu xocol, tu Chicheenitzá u hedz katún; ulom kuk, ulom yaxum, ulom ah Kantenal, ulom *xekik* tu can uadz, ulom Kukulcan tu pach ah itzaob; tu canten u than katún uale.”

2. *Translation:*

4 *Ahau* is the 11th *katun* of the count. In Chicheen Itza they erected the *katun* (stone). There appear the nests, the shoots, there arrived to us Kantenal and the vomit of blood for the fourth time, there arrived Kukulcan with the Itzas for the fourth time they count this *katun*.

3. *MS. de Tizimin, folio 142, vuelta, segundo párrafo:*

“Ucí yabnal u hedz katún ti can ahau katún; tu Chicheen uuc chanal u hedz ti nohol; ah bacocob macan u uich, cimen u uich yokol u nal, yok u uah, chac kiktahan u pop, u dzam; *xekik* u cuch; tu kin yan zazacil a uex, zazac u il a nok, ixchaben uah; hulom kuk hulom yaxum, hulom kaxté, hulom bom, ah tzimin tepoy, mucuc patan tu Chicheen lae.”

3. *Translation:*

Uuciyabnal erected the *katun* (stone) in *katun* 4 *Ahau*; in Chicheen always they erect it to the south. The *milperos* (corn-growers) with their faces covered, with their eyes closed, they lament their meals, with their mat and their bed covered with blood, an epidemic of the vomit of blood, with white trousers, with white clothes, they beg for bread, the nests and shoots appear in the branches of the trees, appear the guano palms, there arrives to us the horse with the money-bag of tribute on the harness, the money-bag of tribute at Chicheen.

## CHAPTER VI

### THE MAYA COUNTRY: COMING OF THE SPANIARDS

#### EARLY EXPEDITIONS

The coast of Yucatan, in which country, at the coming of the Spaniards, centered the Maya power, was visited by Columbus in 1502. His brother, Bartolomé, landing on an island, Cuanaja in the Gulf of Honduras, formally took possession of it. Seven Spaniards of the wrecked expedition of Valdivia from Darien for Cuba, landed near Cozumel in 1511.<sup>1</sup>

Other expeditions came from Cuba: that of Cordoba in 1517, that of Grijalva in 1518, and finally that of Cortez in 1519. These explored the coasts, landed at a number of places, and went up the few rivers as far as there was water to float them, but none went far inland.<sup>2</sup> Narvaez landed at Cozumel in 1520 and left smallpox behind him; he also introduced this disease into the City of Mexico.

Cortez marched across the southern part of Yucatan in 1524 and 1525, on his expedition to Honduras, with a considerable number of Spaniards, some of them "new come from Castile," and more than 3,000 Mexicans. Both of these groups should have been susceptible to yellow fever.<sup>3</sup> It

<sup>1</sup> Five of them were eaten by Indians. One, Gerónimo de Aguilar, "a cleric and a learned man," stayed as a much trusted slave until 1519, when he was taken off by Cortez. The other, Gonzalo de Guerrero, was adopted by the Indians and rose to high rank, War Chief, among them and, Bishop Landa (1864, pp. 12-16) says, "became an idolater."

<sup>2</sup> With all three was our old friend, Bernal Diaz del Castillo, whose rather intimate account of the country is simply invaluable. He was so very human. During the expedition of Grijalva, which went along the Mexican coast as well, as far as Ulua (Vera Cruz): "There were so many mosquitoes on this river" (Coatzacoalcos) "that we went . . . up to a high house of Idols to sleep and near to that house I planted some orange seeds, which I had brought from Cuba . . . and they grew very well . . . the Papas (priests) of those idols took care of them . . . having recognized them for plants new to them." After the Conquest of Mexico and his return from Honduras with Cortez in 1526 or 1527 he was assigned an *encomienda* in this region, looked for, found, and moved his orange trees to it, "where they succeed well." Doubtless these were the first orange trees on the continent of North America.

<sup>3</sup> However, Guiteras says, and truly, that there would have been little chance for a recognizable outbreak of yellow fever in this expedition even had there been



was far from being an unknown country, then, when Montejo seriously undertook its conquest in 1527.

The Spaniards, in none of their expeditions except that of Montejo, nor in fighting backwards and forwards over the country to 1542 and later, make mention of any sickness which can reasonably be considered yellow fever. Possibly the men of the earlier expeditions (prior to 1524) had been in America long enough to have acquired immunity to yellow fever if it were prevalent in such parts of America as they had lived in. If so, its non-occurrence in these men in Yucatan would have no significance as indicating absence of the infection there. This is the explanation offered by Finlay and Bérenger-Féraud, who claim that yellow fever was indigenous on the Mexican littoral. True, the men coming to Yucatan had all sailed from Cuba, then considered a very healthful country and believed to have been free from yellow fever until long after this time (Finlay, 1912); but many of them had come via, and it may be had lived in, Hispaniola or other Spanish settlements in the Caribbean, where it was thought they might have contracted the disease and thus have become immunized to it.

This, however, cannot be alleged to the same extent of the subsequent expeditions. The invasions, after the first one of Montejo's, were all from Mexico; some by sea and some by land. Along with a leaven of the old Conquistadores, mainly in command, they must have contained many newcomers from Spain, whose sole experience of America, except crossing the coast country when they landed, was that in the uplands of Mexico. The emigration from Spain to Mexico had begun early and soon became large. Even in the expedition which Cortez led to Honduras in 1524, for which one would have supposed only picked men would have been selected, there were a number of men "new come from Castile."<sup>4</sup>

---

infected foci in Yucatan, as the march might well have missed such foci. Moreover, one feels like adding that even if they did come upon a focus of infection, the worst that would happen would be the development of yellow fever in the few who became infected during their short stay therein, as the expedition did not stay long enough in any one place for *aegypti* infected there from men in the expedition to become infective and convey the disease to others during their stay. Hence no epidemic, or even serious outbreak, should have been expected.

The same condition of mobility did not exist in Montejo's expedition nor in those subsequent to it, some of which, as that of Montejo at Chichen Itza, formed more or less permanent camps.

<sup>4</sup> For these Bernal Diaz shows all the contempt of an "old timer" plainsman for a "tenderfoot." He especially deplores their inability to forage for them-

## MONTEJO'S EXPEDITION, 1527

Montejo's men were recruited in Spain and hence were neither immune to American infections nor familiar with pioneer conditions of living in the tropics.

En route from Spain they stopped at Hispaniola, where they took on some supplies and where they left their sick, with one vessel. The remainder, apparently about 400 men, landed on the island of Cozumel,<sup>5</sup> but after a few days moved over to the mainland and established there a post or "town," Salamanca, upon a site which all authorities agree was ill chosen, bordering on a swamp. This, says Cogolludo (1688, *lib. ii, cap. v et seq.*), was at the place later called Valladolid: Oviedo (*tom. iii, p. 217 et seq.*) calls it Xala. Here occurred some sickness of which Cogolludo says merely, "some fell sick . . . from the excessive heat." Oviedo, whose account should be more authoritative, says: "There died a great part of the men." He attributes the deaths to lack of provisions and all necessities, to bad water and new airs, and to vampire bats which "killed more than forty!" "Departing from this bad place," says Oviedo, and leaving there forty men "sick and incapacitated," Montejo proceeded about fifteen leagues to Pole (or Polé), "in which place died almost the greater part of the men who remained to him and he was near the end of life." On his departure from Pole, Montejo left there twenty sick men "who were not able to go out" (whom the Indians later killed), and took with him a force now reduced to ninety. With these, he proceeded from place to place along the coast and inland as far as Chichen Itza, returning to Salamanca after some four to six months. By this time his force was diminished to sixty, according to Oviedo, who accounts for not more than ten or twelve as having been killed in battle. Meanwhile, according to the same authority, the forty who had been left at Salamanca had endured such privations that all but twelve had died, making a total of seventy-two survivors. Molina Solis (1896, p. 401-2), whose account agrees with that of Oviedo, but is more detailed, gives the number of survivors as eighty-eight—eighteen in the garrison at Salamanca and seventy returning with Montejo. Soon after

---

selves and reprobates their squeamishness about food. From the latter vice the worthy captain was notably free. He was, too, a most successful and generous forager.

<sup>5</sup> The date of arrival at Cozumel is given by Oviedo as September, 1528. Molina Solis, who cites Oviedo as authority, gives the time as September, 1527. Cogolludo places the departure from Spain in 1527, but states that he is unable to verify the month.



the return to Salamanca, the ship that had been left at Hispaniola arrived, bringing well men and provisions. With these and later reinforcements the Spaniards were able to maintain themselves in the country until 1535, when they were temporarily driven out.

The mortality from sickness is mentioned in the contemporary accounts of two eye-witnesses. The "*Relación of Blas González*," quoted by Molina Solis (p. 382) says: "We were on that coast two months, without entering the land, from which befell many sicknesses and the deaths of fifty soldiers." If the two months include the time spent at Pole, the mortality is considerably less than is implied by Oviedo. A letter from Montejo to the King of Spain, dated at Vera Cruz, April 13, 1529 (*Colección de Documentos inéditos del Archivo de Indias*, 1864-84, tom. 13, p. 86-91), refers to the great difficulties encountered and says: "Some men and horses have died in war, but the most have died of sickness, not through fault of the land, which in truth is very good and healthful, and very fruitful and of many provisions and of very good waters, but . . . on account of the change in rations." This, while confirming the fact of much sickness and many deaths, is a very restrained statement if it refers to such mortality as Oviedo reports. It seems possible, then, that Oviedo's account, though it is sufficiently circumstantial, may be in error as to either the number of survivors or the number originally engaged in the expedition. But even with a liberal allowance for exaggeration, there is sufficient evidence of excessive mortality from sickness.

There is no description of this sickness, either clinical or epidemiological, which serves to identify it. The high mortality, and its occurrence early, in encampments, are consistent with the supposition that the disease may have been yellow fever, but other causes are not excluded. In the absence of more conclusive direct evidence, judgment must rest chiefly on subsequent history in the same region. On this basis, the probabilities are, I think, against this having been yellow fever.<sup>6</sup>

<sup>6</sup> Montejo came by Santo Domingo, where he stopped "for provisions and refreshings:" took aboard fifty-three horses, and left his sick and one vessel to bring them later to Yucatan. So if we think the men of the earlier expeditions were immunized to yellow fever by brief residence in Santo Domingo, and that the sickness in Montejo's forces was yellow fever, we must admit the probability of his men having contracted it there and bringing it to Cozumel. Montejo, himself, was one of the original Conquistadores of Mexico, going in the original expedition of Cortez, in 1519, and had lived in Hispaniola for some years before this. If long residence in America gave immunity to yellow fever, he should have been immune.

## NEGATIVE RECORD, 1528 TO 1648

Whatever may have been the cause of the sickness in Montejo's men, there is no record of anything after this which we can even suspect of being yellow fever in this region until 1648, 120 years later. In the meantime, cities had been founded and immigrants had come from Spain, among them many of the clergy, the causes and dates of whose deaths are frequently recorded. The records show that the Spaniards here considered the country very healthful and congratulated themselves that in Yucatan they were free from the "*ayres maléficos*" known to prevail in Vera Cruz in the wet season and always at Nombre de Dios. Moreover, when the fever of 1648 occurred, although beginning with the Spaniards, it developed as a very general and widespread epidemic among both Spaniards and Indians, and of the old as well as the young. This could only happen if yellow fever either had never visited this region or had been long absent from it, and if this epidemic was the result of a recent introduction.

QUESTION OF THE EXISTENCE OF YELLOW FEVER IN YUCATAN  
PRIOR TO 1648

Did yellow fever exist in this region at the time of the Spanish Conquest? If so, it disappeared entirely during or soon after that Conquest and gave no evidence of its presence for over 100 years. If this region, taken as a whole, was, up to that time, a *permanent endemic focus* of yellow fever, was there anything in the Conquest to make the disease disappear? One would think the contrary. The effect of movements of men in masses is generally to spread infection, and the advent of the susceptible soldiers from the highlands of Guatemala and from Tlaxcala and other parts of the Mexican plateau would especially have had that effect. True, one *can* conceive of a great increase of infection thus produced so completely immunizing a region that yellow fever is ultimately eliminated by that very increase. But there had been many wars in Yucatan before, assumed to have no such effect; and certainly the effect of wars as we have seen them in Cuba, Mexico, and Central America in recent years has been to spread and increase yellow fever. I think, then, we must conclude that yellow fever did not exist among the Mayas at this time, and hence that this region was not a permanent endemic focus of yellow fever.

Could it, at the time of the Conquest and earlier, have formed, with other places, a "regional" or "circulating" focus, within which the infec-



tion, not permanent in any one place, was maintained by continuous transfers from one area to another?

If so, we must face the trilemma: either (a) we must admit this very long time, well over 100 years, as intervening between the infections of Yucatan; or (b) that communication contaminative to Yucatan from the other places which, with it, formed this focus, had lessened at, or after, the time of the Conquest; or (c) that the infections of those other places had about this same time markedly diminished or disappeared and that the diminution of these contaminative communications, or of these infections, or of their joint effect, must have been sufficient to leave Yucatan free from infection for over 100 years.

The first, I think, we must reject. So long an interval would imply so little contaminative communication with infected places that it would hardly be fair to consider them in the same focus, that is, as mutually contaminative.

For either of the last two to occur, the conquests of Mexico, or Yucatan, and of what is now Central America, and the *sequelae* of these conquests are the only new factors introduced. Can we accuse them as causing a disappearance of yellow fever?

The regions in reasonable communication with Yucatan and physically capable of forming part of a permanent focus, that is, of allowing the activity of *aegypti* at all seasons, were (1) the Gulf coast and coastal plain of Mexico, the *tierra caliente*, and (2) the Caribbean coast and lowlands to the east of Yucatan.

For the second alternative, that communication contaminative to Yucatan had lessened, it seems clear that communication between the part of Mexico mentioned and Yucatan should have been not diminished but increased, presumably greatly increased, as a result of the conquests. There had always been commerce by sea between nearby ports, and to a fair amount; but neither the Mayas nor the Mexicans were seafarers like the Caribs, and any considerable intercourse between the Mayas and this Mexican coast would have been by land, on foot, and that without pack animals. The account by Bernal Diaz of the march of Cortez across the lowlands of Tehuantepec in 1524-25 shows how slow and difficult such a trip would be through a wooded country, some of it sparsely settled, and hence with little food.<sup>7</sup>

<sup>7</sup> Many died on this march, especially the Indian porters, and many were left sick along the route. The deaths were all ascribed by our historian, Bernal Diaz, to starvation, hardships, and accidents of the journey. Food was extremely scarce for a long time. This led to their eating their guides—they were from other

The same is true, only to a lesser extent, of the low country of Central America to the east of the Mayas. There was sea traffic in canoes from Yucatan along this coast for a short distance. Further along the coast the natives were of a much lower culture and were not in sufficient number to have been of much importance in continuing the infection of yellow fever. There could, then, have been only limited communication between the Mayas and the coastal plains of either Mexico or Central America before the Conquest.

During the conquest of Yucatan, from 1527 to 1542, there was a constant stream of expeditions from Mexico: some by sea, starting from Vera Cruz, some by land. After the Conquest there was added to this commerce the normal traffic of a seafaring people between open ports. The Spaniards, and doubtless their Indian household servants, would go from place to place by sea. We have record of several of the clergy thus going from Vera Cruz to Campeche. Traffic by vessels, too, would be more apt to convey infection than that by land. Vessels, the crews of which must sometimes have been newcomers from Spain, lay long in the harbors of both of these ports. Vessels, too, would bring passengers quicker than they would come by land, would not leave the sick en route, and, if from an infected port, could bring not only infected men, but infected *aegypti*. The same increase of coastal communication took place from Guatemalan and Yucatecan ports, then including what is now British Honduras, to those to the east, especially Ocoa, Ulua, and Trujillo.

It is not reasonable, then, to think that Yucatan escaped infection on account of the Conquest lessening communication with any place from which it would have received the infection had the Conquest not taken place. Communication increased after the Conquest and, if from an infected place, became more dangerous.

The Conquest also opened communication with Cuba—negligible before—as shown by the different Spanish expeditions of 1517, 1518, and 1519 to Yucatan, of which one at least introduced smallpox. Cuba was probably then free from yellow fever (to 1649), but even if so, there was a chance of susceptible people coming to Yucatan from Cuba via the suspected Mexican ports. At any rate, it was at least a possible, if indirect, source of danger after the Conquest, not existent before.

---

peoples and suitable for eating. This, however, added to their troubles, and even Bernal Diaz, who was in no wise squeamish in such matters, and who had ignored the eating of an occasional porter, confesses this to have been a tactical error (*op. cit.*, *cap.* 175 *et seq.*).



For the third alternative. Was there anything in the Conquest to cause a lessening or a disappearance of yellow fever in Mexico or Central America if it existed in either of these areas? None is obvious. Our experience has been that in any dynastic upheaval involving the mass of the people, strangers are brought into the infected territory and yellow fever increases. There was a less sudden disturbance in the *general* life of the Mexicans by the Conquest than one would have thought, but after the mines were opened, prior to 1544 when Motolinia wrote, there was a shifting of native and Spanish population to the lowlands, where, if anywhere, yellow fever should have been found.<sup>8</sup> Without question, then, such disturbance as was set up by the Conquest, the immigration and movements of the Spaniards, and the vessels with European crews lying in port, all tended to increase and spread any infection of yellow fever already existent in the *tierra caliente* of Mexico and in the lowlands to the east of Yucatan.

To sum up: quite certainly the effect of the Conquest would have been: (1) to increase the prevalence of any yellow fever already existing in Yucatan, in Mexico, or in Central America; (2) to increase the communication between Yucatan and the coast sections of the other two; and (3) if any one of these regions were infected with yellow fever, to increase the danger of its communication to others, as every condition favoring infection and the spread of infection was increased by the Conquest.

Therefore, and since, in fact, the Conquest having taken place, Yucatan was not infected for well over 100 years thereafter, it is not reasonable to claim that it would have been infected during this time if the Conquest had not taken place. The evidence, then, is strong—to my mind convincing—that at the time of the Conquest Yucatan was neither a *permanent endemic focus* of yellow fever nor a component part of a *permanent regional focus*.

One may go further. After the Conquest, even if the direct communication from the Mexican coast to Yucatan were not very free, yet if the Mexican coast were infected with yellow fever it would seem that, in less than the 120 years which did elapse, Yucatan would have received this infection and, if *aegypti* were prevalent there, would have shown an epidemic. It would seem, then, that the decision here must be: (a) the Mexican coast was not all of this time infected with yellow fever, or

<sup>8</sup> Bernal Diaz states that most of the places where gold was obtained, listed in the tribute book of Montezuma, were in the lower country, and thither directed his steps with Sandoval.

(b) *aegypti* were not all of this time prevalent in Yucatan. The first conclusion seems unavoidable in any case, because, if we accept the second, that *aegypti* were not prevalent during all of this time in Yucatan, where the conditions were so well suited for them, it follows almost inevitably that they would be lacking on the Mexican coast as well. Hence, prior to their introduction, the latter could not be infected with yellow fever.

That *aegypti* were prevalent in Yucatan in 1648 goes without saying—else there had been no epidemic. So were they prevalent in Memphis in 1878, but not in 1853; in Fernandina in 1877, but not in 1876. The writer is not denying that they may have been indigenous in Yucatan; only the fact that they were there in 1648 is not proof that they were indigenous.

In any case, it seems to him that with the data we have on Yucatan, one of the above conclusions, (a) or (b), is inevitable, and the first must be accepted in any case since it is implied in the second. Nowhere about the Caribbean do the pre-Columbian conditions for the breeding and existence of *aegypti* seem to have been so good as among the Mayas. If *aegypti* were not prevalent there during this time, obviously they were not indigenous to this region—they could not have died out—but were introduced later. If not indigenous to Yucatan, it is extremely improbable that they were indigenous to the Mexican littoral. That being so, yellow fever could not have originated and been permanently endemic in either of these areas.



## CHAPTER VII

### THE MAYA COUNTRY: THE EPIDEMIC OF 1648 IN YUCATAN

The conclusions from the epidemiological argument of the last chapter are important. The bases of that argument are: (1) that there was an epidemic of yellow fever in Yucatan in 1648, a general epidemic, attacking Spaniards and Indians, old and young alike; and (2) that previously there had been none in that region for a very long time, certainly not since the Spanish invasion of 1527, and probably not since 1517. Let us see if the bases of this argument are secure. On it ultimately rest our conclusions.

For the second part of it—the absence of yellow fever since the Spanish knew the country—we have only the absolute non-report in any historical record until 1648 of anything which can be even suspected of being yellow fever or, indeed, of any epidemic disease save those introduced by Europeans. And this under conditions in which yellow fever would almost inevitably have been mentioned had it occurred. Indeed, unlike Vera Cruz and Nombre de Dios, Yucatan was considered extremely healthful by the Spaniards.

#### THE RECORD OF SICKNESS UP TO 1648

One finds epidemics noted in Yucatan after the Spanish invasion of 1527, but until 1648 all are of European diseases: smallpox, measles, and “*tabardillo*” (sometimes typhus, sometimes scarlet fever, one thinks) mentioned by name. A number of local epidemics of the above, diseases of the chest, catarrhs, etc., are all recorded in the health reports of the different districts during the latter part of the sixteenth century and the early part of the seventeenth.<sup>1</sup>

Besides the great epidemic of smallpox already mentioned as occurring

<sup>1</sup> Among the *Documentos Inéditos de Ultramar*, published of late years at Madrid, are the *Relaciones de Yucatán*, edited by Don José María Asensio y Toledo (1898–1900). These are formal reports, fiscal, sociological, religious, and sanitary, of the different divisions of Yucatan under Spanish rule. The earliest date I have found is 1580, whence they run to about 1620. Many are undated. They are by men far less cultivated than the ecclesiastics, by whom the great majority of our documents of this time were written, and are evidently filled-out forms given to these inspectors. A section of each report is on the state of health

prior to 1528, several others are mentioned in both Maya and Spanish records. Cogolludo, for instance, notes an epidemic of smallpox, measles, and “*tabardillo*” (quite certainly scarlet fever here) in Merida in 1609, which “greatly afflicted the citizens and, to a small extent, the Spaniards.” This is possibly the “smallpox” mentioned in the “Book of Chilan Balam of Mani” (Brinton, 1882, p. 104). Molina Solis (vol. 1, p. 130) mentions an unnamed “*peste*” in 1571, following a famine. This may be the “smallpox occurred” of the “Book of Chilan Balam of Tizimin” (Brinton, *op. cit.* page 142) of 1569, which notes no sickness in 1571. Another “*peste*,” confined to Indians, and hence probably smallpox, certainly not yellow fever, is also noted by Molina Solis in 1561. The negative evidence adduced is, to the writer, convincing that yellow fever was absent during this period.

If the history and epidemiology of the epidemic of 1648 shows (1) that it was yellow fever, and (2) that it occurred in a generally susceptible (virgin) community, we may claim as proven that yellow fever either had never before visited this region or had been long absent.

For the first part of our argument we are exceedingly fortunate. There can be no doubt as to the nature of the epidemic of 1648. Indeed, the writer considers it the first one that we can certainly recognize from its description as being yellow fever. The “*peste*” at Guadeloupe, beginning a month or two later the same year,<sup>2</sup> noted but less fully described by du Tertre (1667, vol. 1, pp. 421–22), is accepted as, and we believe was, yellow fever, as was not unlikely “the Plague or as killing a disease” that Ligon (1673) found in early September, 1647, at Barbados, but of which he gave no description. So *possibly*, but very improbably, was the “*coup de barre*” of Guadeloupe of 1635, which good Père Breton (1665) both notes and describes—of all places in the world—in a Carib-French dictionary.

of the division surveyed. In these *Relaciones*, then, we have an unusually complete and detailed picture of the *sanitary* history of Yucatan for the time covered.

They are rather quaint reading sometimes. Francisco Pecho (only a very few of these inspectors use the *de*, showing that they were not of the upper classes), reporting under date of February 20 (the only date given in days), 1581, expresses doubt of the Christian faith of the Cacique of Cacalchen, “as to his devil he still sacrifices dogs, birds, and some boys (*algunos muchachos*).” (Italics the writer’s.) (*Op. cit.*, vol. i, p. 129.)

<sup>2</sup> It appeared in Guadeloupe about the last part of July, brought there by a ship from St. Christophe. Whether the outbreak at St. Christophe preceded that at Campeche, in Yucatan, or *vice versa*, we do not know. I can find no record of the epidemic of this year at St. Christophe save du Tertre’s note.



For none of these could it be claimed, however, that they were certainly identified as yellow fever by their descriptions. I think we can claim this for the "*peste*" of 1648 in Yucatan, and to that end quote at some length in quite literal translation the account of Cogolludo, as given in his *Historia de Yucathan* (1688, *lib. xii, cap. 12, et seq.*).<sup>3</sup>

#### LOPEZ DE COGOLLUDO'S ACCOUNT OF THE EPIDEMIC

Cogolludo states that the usual portents presaging a great calamity were noted, probably after the event; among them a dense fog, "which seeing, the old Indians said it was a sign of great mortality of people in this land, and for our sins, this came out a certain truth, as in a short time we experienced . . . In the beginning of June the *peste* began in the city of Campeche<sup>4</sup> and in a few days so pressed on it that it was totally laid waste." He quotes a letter from a citizen: "If God does not pity our misery and soon soften the rigor of His justice it will be said 'Here was Campeche,' as it was said of Troy . . . The roads from Campeche were guarded, fearing communication of the contagion . . . With this fear . . . passed the month of July, in the end of which began some people" (in Merida) "to sicken, who died very soon, but it was not recognized to be the *peste* until the beginning of August. With such quickness and violence it came on great and little, rich and poor, that in less than eight days almost the whole city was sick at one time and many of the citizens of highest name and authority in it died. The city, afflicted with such a misfortune *not seen before since this land was conquered by the Spanish nation*<sup>5</sup> . . . sought leave to bring in the Holy Image of Our Lady of Izamal . . .

<sup>3</sup> This account is given more fully and in much smoother, although less literal, English in *Los Trabajos Selectos* of Carlos J. Finlay (1912) pages 214-216.

<sup>4</sup> Before this, he says: "In the months of April and May are seen some deaths, which caused perturbation in the city of Merida." Presumably he means deaths in Merida. If so, this cannot have been the beginning of the epidemic; else, Merida being evidently infested with *aegypti*, the epidemic would have declared itself long before August. This is mentioned here, as are several other unnecessary passages of our writer, in a spirit of fairness, that the reader may have the whole story before him, even if some of it be of little value, and may form his own conclusions. Cogolludo evidently considered the "*peste*" as brought from Campeche to Merida.

<sup>5</sup> "*No vista otra vez desde que se conquistó esta tierra entre la nación española.*" There are no italics in the original, but the writer, following Dr. Finlay (and there is no man whom he had rather follow), is italicizing a number of passages: (1) those that especially show likeness to yellow fever, (2) those which tend to show that this was the first epidemic in Yucatan, and (3) two places pertinent to a Maya text already given (page 125).

"The most part of the Indians of Izamal, who attended the Holy Image on the road and in the City of Merida, were attacked by the contagion of the *peste* in it and a few days after they arrived at Izamal, passed from this present life . . . Some religious companions of the Convent of Izamal, who accompanied the going and return of the Holy Image . . . were also taken with the contagion and of them died those *who appeared most healthy and robust.*"

Whether Izamal was infected by these people, who developed the disease in it, one does not know, yet only those who made the trip to Merida are mentioned as having fallen ill. Molina Solis (vol. 2, p. 175), however, states that "in September all the district of Izamal was infected."

"The tribulation of the city was very great as *never had it experienced such a disaster* . . . In the beginning" (in Merida) "few of the Friars died . . . when the *peste* was at its height few were sick . . . afterwards many sickened at once" (Cogolludo, *loc. cit.*).

The friars lived in cloisters, and at first they would be infected only as they visited the infected houses in town. As those thus infected sickened, they infected the *aegypti* in their cloisters and, after the period of extrinsic incubation in the insects, plus the period of incubation in the men whom the mosquitoes infected had passed (together about 17 to 20 days, usually), they would go down in numbers, as happened at Orwood and Taylor, Mississippi, in 1898. This is quite characteristic of yellow fever; and this interval, during which only a few friars were sick, would be 30 to 40 days, or maybe a few more, from the beginning of the attacks of the men from whom, indirectly, the friars were infected in the city. Our historian has here given us unconsciously an observation which dovetails in with the epidemiology of yellow fever so perfectly as almost to *compel* the diagnosis of it, or some other host-borne disease.

"Pestilences are accustomed to be a common accident in other lands, which uniformly attack all, *but it was not thus in Yucatan*, which was the occasion of the greater confusion. It is not possible to say *what was this malady, because the physicians did not recognize it.*"

In giving the symptoms: "The most common was for the patients to *be taken with a very severe and intense pain in the head and of all of the bones of the body, so violent that it appeared to dislocate them or to squeeze them as in a press.* In a little while after the pain a most *vehement fever*, which



to most occasioned delirium, although to some not. Followed some vomitings as of putrefied blood (*Seguíanse unos vómitos de sangre como podrida*) and of these very few remained alive. To others *there was a flow from the bowels of a bilious humor (humor colérico)* which" (being) "corrupted caused dysentery, which they call '*sin vómitos*'. Others were provoked to them" (vomitings) "with great violence, but in vain, and many suffered the *calentura* and pain in the bones without other accidents. . . . To the most the fever appeared to remit entirely on the third day; and they said that already they felt no pain; the delirium ceased, conversing sensibly, but they were not able to eat nor to drink anything and thus going on one or more days, speaking and saying that they were well, they died. There were many who did not pass the third day, the most died beginning the fifth,<sup>6</sup> very few reached the seventh, except those who survived, and of these the most were elderly. It attacked young men, the most robust and healthy with most violence and finished their lives the quickest. . . . Although very many women sickened, the sickness did not bear as hard on them as on men<sup>7</sup> . . . but it was rare that one found a pregnant woman who remained alive."

The "*peste*," in Campeche in June and July, came "as by a jump to Merida, not affecting any pueblo between them."

"Whilst lasted the force of the *peste* among the Spaniards, the *Indians* did not get sick, only those who were with them" (the Spaniards) "and those who went to the city. These went out touched with the malady and the most part died in their pueblos, but it did not attack those who assisted them" at their own pueblos. "On this account the Indians said with boldness that the malady was a judgment of God, since they were sick only in the city and the towns" (of the Spaniards). "An Indian proclaimed that all the Spaniards of Yucatan had to die and that the Indians alone would remain" . . . . Soon, a few days after the arrest of

<sup>6</sup> This is early. The majority of deaths in yellow fever occur on the sixth day, the great majority on the sixth or seventh. This shows the malignancy of the epidemic, further evidenced by the vomit's being recognizable as blood and hence named *xekik* (blood-vomit) by the Mayas in the "Book of Chilan Balam of Chumayel."

<sup>7</sup> Both Ligon (1673), who saw it, and Scott (1688) noted the greater mortality among men than women of "the Plague or as killing a disease" in Barbados in September–November 1647. The first ascribed it to the drinking habits of the men. The latter stated "as is usual with epidemics, showing a favor to that sex."

this Indian, "*in many of their pueblos the same sickness showed as among the Spaniards, making fearful ravages as among a people without resources or medicines.*" The same explanation would apply here as in the delay of the sickness among the friars.

"The sickness lasted in the whole land for the space of two years. Rare was it that he who was in, or entered into, this land those two years did not sicken, as *neither did they die of recurrences, having gone out of the first accident.* All remained so pallid that they appeared dead: many without hair, with eyelashes dropped out, all so broken that although they had had only two days of fever and a little pain in the bones—as happened to me—many could not regain their strength." This, of course, is not like yellow fever. Weakness is, indeed, extreme in the early part of convalescence, but once established, convalescence is rapid, especially in light and moderate cases, and is complete. Anemia, save from venesection, is not a sequel of yellow fever. The writer has seen the loss of hair and eyebrows in only a few very bad cases.

"As to what I said, that the sickness killed the most robust youths the quickest, I will say . . . that of the children of tender age whom the *peste* attacked in Yucatan, *there were very few who died compared with people of more advanced age.*"<sup>8</sup>

We are fortunate in our historian of this epidemic. He was evidently a man of ability, well educated, and a close and accurate observer. There is no comparison between his account and the note, we can call it no more, of the brilliant and versatile du Tertre on the "*peste*" at Guadeloupe the same year. And there can be little doubt that this was an

<sup>8</sup> Our historian was led to record this characteristic of the "*peste*" by noting, when he left Merida in 1650 on a missionary journey to Guatemala, that in an extensive pine forest "the same year of 1648, in which the *peste* began, some pestilential air or other bad influence had dried up all the pines well grown and large . . . all the new little pines remaining alive, and then I made this reflection, that of the children of tender age," etc. But for this, we would have missed his record of the low mortality of this epidemic among young children, a point of value in determining its relation to yellow fever.

Da Rocha Pitta (1730) in his account of the epidemic of "*a bicha*" at Bahia, Brazil, of 1686, noted the same lower mortality in children and explained it as due to God sparing the innocent, but not their sinful elders. He was somewhat put to it to account for the same phenomenon, and to a greater degree, among the negroes, of whose morality he had the lowest opinion; and rather adroitly turned it by asserting that the sins for the punishment of which "*a bicha*" was sent, were the pride and luxury of the Brazilians, and thus it spared the humble. I judge there were very few negroes in Yucatan, else I am sure that their immunity had not escaped Cogolludo.



epidemic of yellow fever and in a generally susceptible—practically in a virgin—population. One salient point only has he left out, which we should have expected a layman to note—the frequency of yellowness in convalescence. He does not appear to have noted the dead at all.<sup>9</sup> And one thing only has he noted as occurring which is not characteristic of yellow fever—the description of the convalescence. Possibly he intended this word “*pálidos*” to cover the yellowness of convalescence as well as anemia. I know of no other description by a layman, and none by anyone until years later, as complete as this. It is wonderful that in this—the first account of yellow fever recognizable definitely by its description—should be noted so many epidemiological facts, such as the lesser mortality of young children, the greater severity among the robust, the absence of a second attack, its delayed incidence and then sudden outburst among the friars and Indians. Indeed, we may say that not only can this “*peste*” of 1648 be recognized to be yellow fever by its description, but that from its description we cannot fail to recognize it.

Note, too, how new the disease was to every one—“it had not been seen before since the Spanish nation had conquered the land . . . . the physicians did not recognize it.” Cogolludo, too, testified to Yucatan’s reputation for healthfulness: other countries were accustomed to pestilences; “it was not so in Yucatan,” and on this account the blow was severer.

The bases for the argument of our preceding chapter seem, then, to be established, namely: (1) that Yucatan had had no epidemic of yellow fever since the beginning of the Spanish Conquest, 120 years prior; (2) that the epidemic of 1648 was one of yellow fever, and (3) in a virgin people.

<sup>9</sup> The earliest note of jaundice as a symptom of yellow fever that the writer has been able to find (unless Cogolludo’s “*pálidos*” implies it), and the only one until well into the eighteenth century, is by Dr. João Ferreira da Rosa in his “*Trattado Unico da Constituiçam Pestilencial de Pernambuco*,” published at Lisbon, 1694. In *Disputada* i, *Duvida* iv: p. 28, he has: “14. Above all signs there are two dreadful” (fear-carrying) “ones, which are the jaundice (or for another name *morbus regius*) and the suppression of urine. The first an onerous and miserable prognosis” (when) “coming before the seventh day.”

Naturally the designation of the disease as “yellow fever” by Griffith Hughes of Barbados in 1750 presupposes that jaundice had been then commonly noted in that country.

## POSSIBLE ORIGIN OF THE EPIDEMIC

Why did this epidemic of yellow fever appear in Yucatan in 1648, for the first time, as the writer thinks, but certainly after an extremely long absence? What change had taken place either in the trade relations or in the sociological conditions of this country at or near this time which might introduce the infection or render it more capable of propagation? As we have said, the greater intercommunication between Yucatan and the other Spanish colonies, caused by the Conquest, would increase the risk of bringing yellow fever to Yucatan if it existed in such colonies. Indeed, any increased communication from foreign ports would increase the chance of the introduction of yellow fever in proportion to (1) its prevalence in such ports, and (2) the amount and nature of such communication.

Now a new species of maritime communication, the kind most apt to carry both *aegypti* and yellow fever, had sprung up and had undergone great development not long before and during this year. The buccaneers, French and English, mainly, but there were some Hollanders, had begun not only to prey upon Spanish vessels in these waters but to raid the towns close enough to the coast to be exposed to their attacks. And this coast seems to have been particularly troubled by these pirates for some time before. For example, in 1598, William Parker, an English pirate, took Campeche and sacked it. In 1633, pirates again captured Campeche, landing 800 men, and in 1644 Jacob Jackson sacked Champoton, well in the interior. He had 13 vessels and 1,500 men. Jackson returned next year, and captured certain Franciscans, and so on.

Both Cogolludo, writing at the time, and Molina Solis mention these pirates as being unusually prevalent and daring during the year 1648. In April, a frigate from Campeche to Vera Cruz, with more than 100,000 pesos aboard, was captured. The same fate befell a vessel from Spain at Campeche. Later, a Captain Habrahan (possibly Wilbraham, a rather well known English pirate of his time) sacked Bakalar, one of Montejo's Salamancas, and carried off a number of women to a reef ("*los cayos*") in the bay, whence they were finally rescued and brought back by an expedition "of daring youths" *during the pirates' absence*. Certainly this involuntary intercourse with the buccaneers was a new and apparently an increasing factor in the life of Yucatan. Did it place this country, directly or indirectly, in communication with places infected with yellow fever?

The writer has alluded to a general and fatal epidemic, "the Plague or as killing a disease," noted by Ligon (1673), in Barbados in the au-



tumn of 1647. There is nothing to show that this was yellow fever, unless the locality, enabling us to exclude malaria, the most usual epidemic in the tropics, be itself suspicious, and the statements by both Ligon and Scott that the case mortality was decidedly greater among men than among women. This island was much used by English vessels, apparently of all classes, as a port of call and for refitting, etc.

More than Barbados at this time was St. Christophe, the present St. Kitts, a resort for both English and French filibusters. If we follow Père du Tertre, the historian of the early French settlements of Guadeloupe and Martinique, there was a severe epidemic of yellow fever at St. Christophe prior and up to July, 1648. "During this same year, 1648, the '*peste*,' until then unknown in these islands since they were inhabited by the French, was brought here by some vessels; it began at St. Christophe, and in the eighteen months it lasted it carried away nearly one-third of the inhabitants." It was brought to Guadeloupe by a vessel of la Rochelle, called *Le Boeuf*. She arrived at Basse Terre, Guadeloupe, with men sick and dying aboard, and Père Armande de la Paix, the Superior of the Jesuit Missions of the French Islands, went aboard her "to confess and to serve them." He contracted the disease and died on August 4, St. Dominic's Day. The vessel should, then, have arrived at Guadeloupe (Basse Terre) in late July—a few days before or after July 25 (du Tertre, 1667–71, vol. 1, pp. 421–22).

Of the symptoms of this disease, as seen at Guadeloupe, du Tertre writes that it "caused in those who were attacked by it a very violent headache, a general weakness of all the limbs" (prostration), "and continual vomiting, so that in three days it would send a man to his grave." We have no description of this "*peste*" as it prevailed in St. Christophe.<sup>10</sup>

<sup>10</sup> Both Père Pelleprat (1655) and the historian of the Jesuit missions in the French Antilles and Guianas (1857) give accounts (that of the latter based on Pelleprat's narrative) of an epidemic sickness at St. Christophe in 1652 and 1653, associated with so great a scarcity of food as almost to merit the designation of famine. No description of the disease is given, save that it was communicable, fatal, and general over the island. One priest died of it and another barely escaped with his life. Pelleprat went to St. Christophe in 1651 from Guadeloupe, and does not mention any previous sickness in St. Christophe. Of the two concurrent calamities, he evidently considered the scarcity of food (*disette*) the more serious, and it is not unlikely that a severe epidemic of yellow fever, "taking off one third of the population" in 1648, would have produced a great scarcity of provisions—a thing not unusual in St. Christophe—for a year or so afterwards, while a recrudescence of the fever, possible from the arrival of newcomers, would not be very serious, there not being enough susceptible material for an epidemic. There was, however, unquestionably a sickness, fatal, considered communicable, and fairly general, at St. Christophe with this famine in 1652 and 1653.

## CHAPTER VIII

### HAYTI (HISPANIOLA)

The regions of America which we have already examined for the pre-Columbian existence of yellow fever are those which are most generally—we think universally—accused by the advocates of the American origin of the disease, and are unquestionably those which at that time gave the best conditions, climatic and sociological, for being permanent endemic centers of that infection. Yet it seems advisable to examine such other regions of America in which the pre-Columbian—or at least very early—existence of yellow fever has also been claimed.

#### ARGUMENT OF FINLAY, BÉRENGER-FÉRAUD, AND OTHERS

The protagonists for the American origin of yellow fever have been, in chronological order: Moreau de Jonnés, Cornilliac, Finlay, and Bérenger-Féraud, the last two writing at about the same time. All four believed that yellow fever occurred early in Hispaniola ("Española"), causing the great mortality during the "hunger" which followed the battle of Vega Real, March 24, 1495. The first two writers believed it endemic in this island prior to the arrival of the Spaniards (1492). Their belief, which was general at that time, was that the cause of yellow fever was a telluric emanation or an organism locally saprophytic, but facultatively parasitic to man, producing the disease in susceptible men exposed to it, but existing in places under the above conditions independently of its manifestation among men; and existing indefinitely unless destroyed by some agency such as frost, fire, etc. They would probably, then, have held that the cause of the infection of yellow fever was *produced* in Hispaniola by local telluric and climatic conditions, independently of its population, in geologic times.

Finlay, however, believed that it was introduced in 1495 by the Caribs from the Lesser Antilles or the mainland, Hispaniola having been previously free from it. Prior to this, there had been much sickness with a number of deaths among the Spaniards, and the sickness was markedly increased during the famine; but Finlay was compelled to his belief that the disease was newly introduced into the island by the fact that the sickness and mortality among the Indians in the outbreak of 1495 were greater even than among the Spaniards. The fact that no yellow



fever, or other malignant disease, was reported in the Carib Islands (the Little Antilles and Porto Rico) or in the Guianas, when first settled by Europeans, renders this view improbable. Nor, if yellow fever existed among the Caribs, is it clear why it should not have been introduced into Hispaniola earlier. The Carib settlement (Higuey) on its south coast, while not an original Carib seat, was over a generation old when Columbus came and was presumably in ordinary communication with the other Caribs. Yellow fever might, of course, have been introduced earlier, have died out from failure of the human host, and have been accidentally reintroduced; but one knows no special reason why this should have happened just at that time, although, if the disease were introduced at that time, the sociological perturbation caused by the coming of the Spaniards would have caused it to be more readily propagated.

#### SICKNESS IN COLUMBUS' COLONY

There was sickness in Hispaniola from the beginning. Soon after Columbus' colony landed, December, 1493, "the people began suddenly (*de golpe*) to fall sick and, for the little coolness that they had, to die also many of them so that scarcely remained a man of the hidalgos, or of the plebians, however robust he might be, who did not fall sick of the terrible *calenturas*. . . . Nor *did the Admiral* escape."<sup>1</sup> This from Las Casas (vol. 2, p. 22). Herrera gives practically the same account. Dr. Chanca, the physician of the expedition and an eye-witness, in a letter to the Cabildo of Sevilla, reports the same prevalence of sickness, but clearly does not consider it to be generally of a severe character: "One third of them sick . . . but I hope in Our Lord that all will be raised up in health."<sup>2</sup> Columbus, too, witnesses to the prevalence of this sickness, but is inclined to belittle its severity. In a letter to the Sovereigns of Spain, dated January 30, 1494, explaining why he is not sending more gold by Torres, who sailed February 2, 1494, he says: . . . "of the people, who here are ours, the greater part suddenly fell sick," but "Confiding in the mercy of God," he believes that "this people will convalesce very soon, as already it is doing (*ya lo hace*) and immediately

<sup>1</sup> Columbus was sick again, and much more severely, from a "*modorra*," "nearly deprived of his senses," some months later aboard ship off Mona. There was no other sickness recorded as occurring on the vessel. Finlay is inclined to consider this yellow fever. I would be glad to if I could. For Columbus to be the first European to have yellow fever would be at least dramatic.

<sup>2</sup> Antonio del Monte y Tejada, *Historia de Santo Domingo*, 1853, p. 298.

. . . . if they had some fresh meat in order to strengthen them, very soon all would be on their feet with the aid of God, and even the most are already convalesced in this time," and adds that he is purposing to build a wall around the settlement "with these men as they get well and the few who remain well" (Del Monte, p. 257).<sup>3</sup> His memorandum to Torres, too, gives: "Say to their highnesses, as it is said," (in the above letter) "that the cause of these sicknesses, so general of all, is the change of water and airs, because we see that it extends to all successively and they endanger few (*peligran pocos*)."<sup>4</sup>

The work on the wall of Isabela was not stopped. The men, "hidalgos and common men (*gente de trabajo*), and even men of black capes (*capas prietas*—courtiers), being driven to the work with violence," by the Admiral, "in spite of sickness and weakness from sickness." Finlay is unquestionably right. This was *not* yellow fever!

#### SECOND EPIDEMIC, 1495

Later, especially after the food became scarce, the sickness increased in severity. Food became scarce rather early, and at times very scarce, it being noted in March, 1494, that there was no more flour. The provisions brought from Spain got wet and spoiled. They were too many (1,300, says Del Monte) to live off the country, nor were they generally the class of men suited to found a self-supporting colony, there being many hidalgos, courtiers, and adventurers. Also, unfortunately, gold was discovered in some of the streams, though at a distance of 20 leagues from the settlement, and the search for this lessened the inclination of the company to the agriculture so necessary for sustaining the colony. The mortality from the sickness increased as time passed. The scarcity culminated in the famine of 1495, and with it the sickness, or at least the severity of the sickness.

The Indians, after their defeat at Vega Real, March 24, 1495, destroyed their crops and planted no more, with the intention to drive out the Spaniards by famine,<sup>5</sup> "they themselves taking to the woods and

<sup>3</sup> Navarrete (vol. i, p. 225 *et seq.*) also gives this letter.

<sup>4</sup> Chanca regarded the bad voyage ("*mala pasada del camino*") as a factor in this sickness, and it may be that there was a substratum of scurvy to it. Scurvy was not then recognized as a disease. The first note of it was of its appearance in Vasco da Gama's voyage of 1597–1598.

<sup>5</sup> "Not knowing the quality of Spaniards, that as their hunger increased, so much the more did their tenacity, and the more they suffer the harder they are to make suffer" (Las Casas, vol. ii, p. 105).



living on wild things," such animals as they could find, mainly rodents and reptiles, wild fruits and roots. The Spaniards were less able to do this than the Indians, "not knowing what to eat and eating hurtful things," and they suffered greatly. By this famine and the sickness accompanying it, both Las Casas and Herrera state that more than one-third of the Indians died.<sup>6</sup>

As to the nature of this second epidemic, the *sequelae* of this sickness presented in the men returning to Spain, on which the French writers rely for their diagnosis of yellow fever, are given on page 74 of Part II, and, as there stated, indicate malaria, not yellow fever. Finlay bases his opinion that it was yellow fever on the mortality among the colonists. This was unquestionably heavy. Of the 1,500 who had come with Columbus in 1493, only 300 remained in 1502, a loss of 80 per cent in nine years. Del Monte, in general more accurate than Las Casas, from whom this figure is taken, gives the number in 1493 as 1,300, which would make the loss a little less than 77 per cent.

#### SUBSEQUENT HISTORY OF HISPANIOLA

With Ovando, in 1502, came 2,500 men who "hastened into the mines to collect gold"—"a new work to them"—and "on account of fatigues, lack of food, and the heat, they took sicknesses and fevers and more than 1,000 of them died," that is, 40 per cent of these newcomers. These losses are heavy, but considering the difference in climate and the severity of the famine, the loss is, if anything, less than at Jamestown; and it does not seem necessary to introduce yellow fever to explain it. In Hispaniola *Anopheles* would be functionally active the whole year; in Jamestown not over four and a half months, at most.

Las Casas is the only writer who ascribes any considerable number of deaths of these Spaniards to violence. He says some were killed by fighting among themselves over Indian women; some by the Indians for the same cause. Many went off into the interior with women—"even with two women," he records with horror—and did not return. "Nor did they return who, looking for gold, went into the territory of Caonabo." The other historians give instances of small parties, looking for gold, etc., cut off in this latter district, inhabited by Caribs. It would, I think, be a proportionately small number, however.

Later, Isabela was moved to a less marshy site—which would not have

<sup>6</sup> In early Spanish writings nearly all epidemics in the Americas are reported as "carrying off one-third of the people"—no other fraction is ever used. The same is true, though not so universally, in the early French accounts.

affected yellow fever—and we hear little of its unhealthfulness, even to newcomers. At no time prior to 1649 is any mention made of epidemics on vessels in the harbor, or going therefrom, or in expeditions, like those of Ojeda, Nicuesa, or Montejo, stopping there some little while. The absence of reports of epidemic sickness on shipboard seems, to the writer, significant.

There seems, then, no sufficient ground to believe that Hispaniola, or places in contaminative communication therewith, were infected with yellow fever at the time of the discovery of America, or indeed until long afterwards.<sup>7</sup>

No one has predicated the existence of yellow fever in any of the Greater Antilles except Hispaniola at the time of their settlement by Europeans; and indeed Finlay, the most earnest and erudite of the advocates of the American origin of yellow fever, is very definite in asserting its absence from them.<sup>8</sup> Hence, to discuss the history of the discovery and settlement of these other islands, in which the writer cannot find one iota of evidence for the existence of yellow fever, would seem like defending a position not attacked—the destruction of a man of straw.

The above, taken with the fact that after the elimination of Havana as a permanent endemic focus, yellow fever spontaneously disappeared from all of the West Indies, although these islands had then a larger population and more communication among themselves and with the outside world than in pre-Columbian times, is, the writer thinks, sufficient to exonerate them from having been a permanent endemic focus in pre-Columbian days.

<sup>7</sup> It may be somewhat significant that the mortality was high among the negroes Saco (1879, *Historia de la Esclavitud de la Raza Africana en el Nuevo Mundo*, page 67) quotes a letter from the King (in Munoz' "*Colección*") to one, Sampier, in Hispaniola, dated Seville, June 21, 1511: "I do not understand why so many blacks have died" (in Hispaniola). "Care for them much." They were later very healthy—"not dying except when hung."

<sup>8</sup> "From this we deduce that before the discovery, yellow fever was endemic in the coasts of the *Tierra-firme* and in those of the sea of the North of New Spain" (Mexico), "but not in the islands of Santo Domingo, Jamaica, Cuba, nor probably in Porto Rico nor in Trinidad; but that in the first of these islands, Santo Domingo, the sickness was imported in the years 1494 or 1495" (Finlay, 1912, p. 121).



## CHAPTER IX

### THE ISTHMUS OF PANAMA: EXPEDITIONS OF OJEDA AND NICUESA

Both Finlay and Bérenger-Féraud lay greater stress on the early existence of yellow fever on the *tierra firme* than in Hispaniola, and for this they rely on the history of the expeditions of Ojeda and Nicuesa to the Caribbean coast of Colombia and Panama. They base their belief on the statement that the men in these expeditions were newcomers from Spain and suffered great, indeed excessive, losses from sickness during these expeditions, while in the same regions there were practically no losses at all among the men with Balboa, consisting of the veterans of these expeditions and men recruited in Hispaniola; and that later, when a large body of men directly from Spain settled in the region in which Balboa's force had been enjoying good health, they again suffered heavy losses from sickness.

If the facts are as stated, it is unquestionably an allowable deduction that the sickness was one of which one attack on the Isthmus had produced immunity, and that those resident in Santo Domingo were immune by previous attack of the same disease in Santo Domingo. Let us examine the facts.

#### HISTORICAL AUTHORITIES

Rather full accounts of these expeditions are given by Herrera, Las Casas, and Oviedo. Although agreeing in general, they differ in many details. There are a few side-lights also from other sources, as in Navarrete's *Colección* of original documents of Spanish discovery, etc.

*Herrera* was the official historiographer of the Spanish Court for the New World. He thus had access to all official documents; and from them, and with much classical and ecclesiastical learning, his history is written. His dates, then, are doubtless authoritative (few are given by either of the others), as are the broad features that would be given in such papers. If, however, the official documents of this period, as given in Navarrete's *Colección* and in *Los Documentos Inéditos del Ultramar*, are a criterion, one wonders where he got the detailed data for some of his dramatic descriptions, as of affairs on Escudo Island or of the "modorra" at Darien.

*Las Casas* was in Santo Domingo (Hispaniola) when these expeditions outfitted there in the autumn of 1509. Of this he says: "I saw what I have related (*yo vidé lo que he contado*)."<sup>1</sup> He is authority, then, and the only eye-witness for what occurred there. He was never on the Isthmus of Panama, the scene of these expeditions, and could scarcely have had good opportunity of getting information directly from the men who took part in them. Moreover, his account of them was written in 1559,<sup>2</sup> forty-nine years later, when he was eighty-five years of age, long enough after the events and he old enough for any supplementing of records by memory to be of doubtful reliability. The frequent identity of passages in his book with those in *Herrera's*, and their very general agreement in the number of men lost on different occasions by these expeditions,<sup>3</sup> leads one to infer that he had access to *Herrera's* manuscript, or it may be, only to the same documents from which *Herrera's* was written. He was in Spain from 1547 to 1566, when he died. He was in Spain, then, when he wrote this, and frequently, in his capacity of "Protector of the Indians," went to Court and appeared before the Council of the Indies, from the records of which *Herrera* was writing.<sup>3</sup> He makes several allusions, all disparaging, to "what Oviedo has written," asserting that it "contains as many lies as pages," and had evidently seen the greater part of his book. As Part I of Oviedo's work was published in 1535, and Part II with Part I revised, in 1547, he may well have done so. Also a "Summary Report" of Oviedo's history had been published at Toledo in 1526, covering, naturally, only so much of it as was then written.

*Oviedo* enjoyed some peculiar advantages for his task. He was brought up at the Spanish Court and later served on the staff of "the Great Captain," Gonsalvo de Cordova, in Italy. He was early interested in the New World and says that he "formed the intention" to write this history in 1493, being then 15 years old. The Spanish Court was then the center of information on the Indies, and his privileged position—he was groom of the chamber and friend to Don Juan of Austria—gave him access to the men who had this information. He mentions and quotes Peter Martyr, with whom Columbus carried on a personal correspondence during, and concerning, his earlier voyages.

<sup>1</sup> "*hasta hoy que es el año de 1559*" (*Las Casas*, v. 3, p. 387).

<sup>2</sup> When they differ in this *Las Casas* gives the larger number.

<sup>3</sup> *Herrera's* great work was published in 1601–15, but it was written some time earlier. *Las Casas' Historia de las Indias*, though completed in 1561, was not published until our own times, 1875–76.



He first went to the New World with the expedition of Pedrarias, and landed at Darien June 30, 1514, only three and a half years after Nicuesa left that place to his death. He was stationed in this government for a number of years, representing the King as "*Veedor de las fundiciones del Oro* (Inspector of the Productions of Gold) *de la Tierra Firme*," and was a thorn in the flesh to Pedrarias. A little later he served here in addition as Supervisor of the Clerks of the Judicial Records, representing Lope (de) Conchillos, "the Secretary."<sup>4</sup>

Oviedo evidently, then, was in the region covered by these expeditions soon after they were made and, although not an eye-witness, had opportunities of talking with a number of those who took part in them, and who were still at Darien and Nombre de Dios. He would have been induced to seek information at first hand by his interest in New World affairs; and the fact that he was a soldier would make it easy for him to approach and to win the confidence of the Spanish soldiers engaged in the expeditions, while his knowledge of the men, and, to an extent, of American affairs, would help him to separate the true from the false in their tales. Again, there is intrinsic evidence in this history that he did get such information directly from men who had been on these expeditions—for instance, giving the number of corpses counted in the pile of dead at Matarap and the statement that they were swollen from the poisoned arrows; that the leaf in which Nicuesa's note was found wrapped was an "*annato leaf*;" that the "*almendras*" eaten on the island were, from the description, not that nut but "*caperas*;" the account of the "assiduous" hound with Nicuesa's party following the deer into the sea and bringing it back by the ear; giving the name ("Leonçico") of Vasco Nuñez' dog; that he "had seen and had read the oath" to forbid Nicuesa's landing, "subscribed to" in the church of San Sebastian at Darien, and "afterwards was acquainted with the most of the subscribers."<sup>5</sup>

<sup>4</sup> Of the Council of the Indies, one thinks. Orders of this Council are signed "by order of the King, Lope Conchillos."

<sup>5</sup> The subjoined passage, too, could have been written only from the narrative of some one present at the episode it relates; it may be that of Balboa himself:

"And one Tuesday, the 25th day of September of that year, 1513, at ten o'clock of the day, the Captain Vasco Nuñez, going in advance of all those whom he was leading up a bare mountain from the highest point of the summit of it (*desde ençima de la cumbre*) saw the Sea of the South before any of his Christian companions who went there, and turned incontinently his face (*volvióse incontinente la cara haçia su gente, muy alegre*) towards his people, very joyful, raising his hands and eyes to heaven, praising Jesus Christ and His Glorious Mother, the Virgin,

His official position on the *tierra firme* gave him probably as free access to official documents as had the two churchmen,<sup>6</sup> and his location, profession, and point of view gave him a decided advantage over them in other ways, he frequently having direct access to the information on which such documents were based.

Oviedo must also have revised his work very carefully, as inconsistencies are practically absent from it, while they are common with Las Casas, and occur occasionally with Herrera. One cannot but consider that this part of Oviedo's *Historia*—whatever may be said of some of that relating to the conquest of Mexico—is by far the most reliable and authoritative account that we have; and it is, in general, the one followed in this paper. It is really a great history.

However, as has been said, all three historians are in general agreement as to these expeditions. In the details, the two ecclesiastics seem inclined to stress the horrific, and indeed the sensational accounts which they give of the sufferings of the men of these expeditions (mainly from hunger) and their hysterical reactions thereto, make one believe that there is an element of imagination mixed with the historical data which they had. Their accounts of the behavior of the Spanish soldier under stress are entirely different from those given by military eye-witnesses such as Bernal Diaz.

#### PREPARATIONS FOR THE EXPEDITIONS

A concession was given by the King of Spain, June 9, 1508—Oviedo says he read it at Burgos, and it is given in Navarrete's *Colección*—to Alonso de Ojeda and Diego de Nicuesa to settle and govern part of the *tierra firme*. To the first was assigned "*Nueva Andalucía*," extending from the middle of the Gulf of Uraba (Colombia) east to Cabo de la Vela in Venezuela. To Nicuesa, "*Castilla del Oro*," from the same starting

---

Our Lady, and immediately bent both knees to the ground and gave thanks to God for the grace which He had shown him."

Indeed, the whole account of this expedition of Balboa (*lib.* 29, *cap.* 111) might have been written by Balboa himself, so vivid and so sympathetic is the picture it presents, and so full and artistic the detail necessary to make it so. That some of the historian's information was directly from Balboa is evidenced by the not infrequent occurrence in his book of the phrase, "thus was it said by Vasco Nuñez." Oviedo greatly admired Balboa and rightly considered the discovery of the "Sea of the South," next to that of Columbus, the most important one made in the New World.

<sup>6</sup> He states: "All the writings of Vasco Nuñez came before me (*á mi poder*)"—doubtless as Supervisor of the Clerks of Record.



point west to Cabo Gracias à Dios in Honduras. The Province of Veragua, in the western part of the present Republic of Panama, was included in Nicuesa's territory, although it had been previously assigned to Columbus and his heirs. The joint use of Jamaica, for provisioning and as a source for slaves, was also granted. This, too, had been discovered by and assigned to Columbus, and both this island and Veragua were claimed by Diego Columbus, the son of the Admiral, then resident as *Adelantado* in Santo Domingo. He, then, bitterly resented this new assignment.

Juan de la Cosa, a pilot of high repute in American waters, was appointed by the King second in command in Ojeda's expedition. He soon arrived at Santo Domingo from Spain with 200 men, one ship and two brigantines, for Ojeda, who was already there.

Nicuesa arrived a few days later and, "because he was rich," he had a larger fleet, four large ships and two brigantines, "and a greater equipment for men." He had stopped en route at the island of Santa Cruz and kidnapped one hundred or more Carib Indians to sell as slaves in Santo Domingo.

Both Ojeda and Nicuesa enlisted men in Santo Domingo to fill out their complements, purchased horses, and outfitted generally for the expeditions. There was considerable bickering between them, about their boundaries and other matters; and the *Adelantado*, Diego Columbus, put as many obstacles in their way as possible, having Nicuesa arrested, for debts and pretended debts, at least three times, and very nearly succeeding in preventing his sailing at all. He did succeed in holding Ojeda until the *Adelantado* had himself outfitted an expedition, under Esquivel, for the settlement of Jamaica, thus to sustain his claim to that island.

Ojeda finally sailed for the *tierra firme* November 10, 1509, with 300 men and 12 horses. Before leaving, he had arranged with the Bachelor Enciso, a wealthy lawyer, to enlist more men, fit out another vessel, and follow him some months later with provisions and reinforcements.

Nicuesa also recruited men in Santo Domingo and "because he had the greater armada and was gracious of manner and well liked, and Veragua" (where he intended to settle) "was regarded as much richer in gold than Uraba, he raised many island-men (*isleños*)," more than Ojeda, "so that, in addition to the ships he brought from Spain, he had to buy another." He sailed November 22, according to Las Casas, with 700 men and six horses,<sup>7</sup> having made the same arrangement with

<sup>7</sup> He is accredited later, by Colmenares, when speaking of his losses at Nombre de Dios, with having brought 780 men from Santo Domingo and, in the same con-

Rodrigo de Colmenares as Ojeda had with Enciso, and having appointed him as lieutenant, or at least so Colmenares stated after Nicuesa was dead (Navarrete, vol. 3, p. 386).

Ojeda obviously added 100 men to his quota in Santo Domingo. How many Nicuesa added we do not know. It was more than Ojeda's, and, from the increase in his armada, should have been considerably more. Colmenares states of Nicuesa's party: "they sought to raise 400 men in this island of Santo Domingo," but we do not know that they succeeded. "He enlisted very many people in this island," says Las Casas, who was on the ground. Places on these two expeditions were evidently eagerly sought by men in Santo Domingo. Pizarro joined Ojeda, Cortez would have gone with them but for a swelling on his leg, and Balboa followed with Colmenares. Clearly, then, the first allegation of Finlay and Béranger-Féraud, that the personnel of these expeditions were men new-come from Spain, is measurably true, but not entirely so. The basis, and doubtless the majority of them, were newcomers, but there were a considerable number recruited in Santo Domingo.

There is no mention of any sickness among these men at Santo Domingo or en route to the *tierra firme*. There is no record of how long they stayed in Santo Domingo; but from the preparations they had to make there, and the trouble with the *Adelantado*, it was probably some weeks. The voyage itself was only a matter of four or five days, says Las Casas. Let us follow their history.

#### OJEDA'S EXPEDITION

Ojeda arrived off Cartagena and prepared to land. Juan de la Cosa advised him not to do so at this place, but to make his settlement "on the right side" of the Gulf of Uraba, which, of course, would have been in Nicuesa's concession, "because the Indians on this side were very savage, skilled archers, and used poisoned arrows," implying that this was not true of those on the other side of the Gulf, which indeed was correct, this Gulf marking the western limits of the Carib country. Moreover,

---

nection, Las Casas and Herrera say 785. Yet in their accounts of his setting out, both Herrera and Las Casas put his force at 700, and the latter says: "I saw what I have related."

In a detailed account of the distribution of his force the day after he reached *tierra firme*, after stating that none had been lost at this time, Oviedo gives the total as 650, and this is believed to be correct as the number that sailed from Santo Domingo with him.



"they were irritated against the Christians, as they had been ill treated by Christobal de Guerra."<sup>8</sup>

Ojeda, however, landed where he first intended, attacked, and captured with little resistance, a small pueblo of the Indians ("Pueblo de las Ollas," Oviedo calls it; Herrera and Las Casas give other names). No account is given of his losses in so doing, but it is related that "some of ours were wounded by arrows" before they could capture a cabin from which some bowmen were fighting,<sup>9</sup> and that four were wounded by the arrows of an Indian Amazon of sixteen to eighteen years. Probably some of the wounded died. Las Casas states that some were killed here, but gives no number.

Following this, Ojeda marched about twelve miles inland to attack a large pueblo, called "Matarap" by Oviedo; "Yurbaco" and "Turbaco" by Herrera and Las Casas, respectively. Having made camp carelessly, he was surprised, beaten, and driven back to his landing place. His loss in killed is given by Oviedo as "up to 100 men and . . . . Juan de la Cosa."

Las Casas and Herrera state that every wound by the poisoned arrows of the Caribs, no matter how slight, "even a pin-prick," so blood was drawn, was fatal. However, Oviedo, a soldier, says he has seen cases recover when the poison was no longer fresh. It must have been fairly fatal, however, for of 47 of Colmenares' men wounded by arrows at Santa Marta it is stated that only one escaped death (Las Casas).

Next day Nicuesa appeared, and with 300 of his men and "some" of Ojeda's, the attack on Matarap was renewed. The pueblo was surprised before dawn and taken. There is no note of any loss to Ojeda in this attack, and there probably was none, as Oviedo states that only three of Nicuesa's men were wounded by arrows and that none died. It is here that he speaks of the occasional recovery from wounds with Carib arrows.<sup>10</sup>

<sup>8</sup> La Cosa spoke from first-hand knowledge of this. Monroy, with one vessel, had been wrecked here, and de Guerra and la Cosa, who had gone to his rescue with two vessels, were also wrecked and with more than 50 men remained, marooned, on this coast for two and a half years. Both the above were slave-seeking expeditions, and these were not the only ones. Some "irritation," then, was not unnatural.

<sup>9</sup> Oviedo says some pieces of gold had been placed before it, knowing the desire of the Spaniards for that metal—i.e. as bait.

<sup>10</sup> Herrera and Las Casas both give Ojeda's loss in his defeat at 70 men, but I have thought it safest to follow Oviedo, partly for the reasons previously given, and partly because he notes that in the captured pueblo "there were counted 80 corpses of Spaniards" in a pile, "all swollen by the poison of the arrows." Evi-

Nicuesa left at once for Veragua, allowing his men to take none of the plunder from the captured town. He had stayed on Ojeda's territory only about thirty-six hours.

Ojeda, still in Carib country, constructed a fort of "thick timbers," a stockade, one judges, and built huts, thirty or more, therein. This he dedicated to St. Sebastian, whose protection he sought against the Carib bowmen with their poisoned arrows. Making a raid therefrom, for gold, against a *cacique* named Tirufi, he was defeated and followed back to the fort. In this fight, says Las Casas, "many of Ojeda's men, wounded by arrows, died raving." Food having become very scarce, he made another sortie to obtain some, but "his attack was met by arrows like rain and he was routed and followed until the gates of the fort were shut . . . and they had hard work therein to bury the dead and to care for the wounded and the few who escaped the deadly poison." (Las Casas).

Some time later, nearly starving, and "some sick from eating harmful things,"<sup>11</sup> they made another sally, again to meet with defeat and pursuit to the very gate of the fortress. In this Ojeda was wounded. His recovery is ascribed to burning out the poison with a bar of white-hot iron; but as he was shot through the calf of his leg, his raw-hide leggings may have wiped the poison from the point of the arrow. Hereafter, he and his men stayed in this fort, "not daring to go out of the fortress a step to seek provisions, because they were severely punished (*escarmen-*

---

dently this is an account he received from an eye-witness. Also, though of less weight, he quotes Ojeda, as if verbatim, telling Nicuesa: "I have lost Juan de la Cosa and 100 of my best men." Note, too, that only some of Ojeda's men joined in the second attack on Matarap, Nicuesa furnishing 300. It would seem then, that Ojeda had not over 200 men remaining, for some of those wounded by poisoned arrows at the Pueblo de Ollas had probably died.

<sup>11</sup> Through all the early accounts of great scarcities of food and the accompanying sicknesses among the Spaniards in the New World, runs the belief that much of the latter was due to "eating harmful things"—i.e. poisons. One is inclined to accept this with considerable reserve, yet along with a number of edible fruits, seeds, roots, etc., growing wild in the American tropics, there are others which are poisonous; e.g. the *manzanillo*, which has rather a pleasant taste; *peras del mar*, which no man in his senses would eat; *piñas bravas*; *caña fistula*, something like the fruit of the honey locust; the root of the wild cassava, etc.; and it may be that some credence should be given to these statements. Dampier tells of a rather wholesale poisoning of a party of buccaneers, in 1681, by eating *manzanillos*. This was in the San Blas lagoon, not far from where Ojeda was at San Sebastian—and the buccaneers were in no wise scarce of food—merely picnicking on the beach.



*tados*) by the poisoned arrows, until from hunger and from harmful foods, and from sickness," they were in great extremities (Las Casas, v. 3, p. 300).

After about four months of this (date not given), a small vessel, stolen by Bernardino Talavera, loaded with cassava bread and bacon, came in from Santo Domingo and temporarily relieved their distress. When this food was exhausted, Ojeda gave up and left for Santo Domingo, leaving Pizarro in command, and promising to bring or send relief within fifty days.<sup>12</sup>

After waiting, starving, and skirmishing until past fifty days, "during which time had died one half of the men left by Ojeda," Pizarro got ready to leave for Santo Domingo. The men remaining—given as 70 by Herrera and as 60 by Las Casas—were too many for the two brigantines they had left, so "knowing that in a short time they would be sufficiently reduced by hunger, sickness, and the arrows of the Indians, they waited" and in a few days, we are told, there was room aboard for all of them.<sup>13</sup>

As they were leaving the harbor, one of the brigantines foundered, and all aboard her were lost. The other, with Pizarro, had just cleared the harbor when Encisco was seen coming in, according to agreement, with provisions, equipment, and reinforcements of 150 men. With him came Balboa.<sup>14</sup> This was "six months after they had entered this Gulf," says Las Casas.

Let us examine Ojeda's losses. He left Santo Domingo with 300 men, 100 of them recruited there. When he landed to build San Sebastian he

<sup>12</sup> Herrera says that Ojeda never reached his destination, but died, in the garb of a Franciscan, presumably in Cuba. Las Casas and Oviedo make the same statement as to the assumption of the ecclesiastical robe, but say that he reached Santo Domingo. "He was in this city many days after this," says the former, "and I believe for even a year. I saw him there (*Yo lo vidé*).'" Both these ecclesiastical historians, Las Casas and Herrera, greatly praise and congratulate him on his pious end.

<sup>13</sup> This statement is possibly due to a desire to paint a very dark picture. There are several instances given of brigantines at this time carrying, as transports, from thirty to thirty-four men as their ordinary complement, and the writer recalls none mentioned as carrying less than twenty-eight; and unless these two were unusually small, one would think that they would have crowded a few more on each one above the normal complement, and not waited for the excess to die off or be killed. It was only a short voyage from Santo Domingo, and the excess, accepting the figures of Herrera or Las Casas, must have been small.

<sup>14</sup> He came surreptitiously. Oviedo says hidden in the bight of a sail; Las Casas, in a barrel; and, although we have all seen the picture of his bursting out of the barrel, the first is at once more dignified and more probable.

had lost 100 men in the attack on Matarap, and probably a few at Las Ollas. There were three disastrous raids attempted from the fort at San Sebastian. The number lost is not given, but the loss in the raid against Tirufi and in the first one made for food must have been heavy; and the sortie in which Ojeda was wounded was made by men weakened by hunger. The statement that the Spaniards feared to go outside of the fort for food on account of the arrows implies, too, that some did go out and were at least shot at. One would think, then, though no such numbers are given, that the loss from fighting during the three raids would not be less than in the one fight at Matarap, that is, about 100 men. As 70 (or 60) is given as the number ready to sail with Pizarro, this would leave only 30 or 40 deaths from other causes to be accounted for—probably a few less. Considering the privations endured, this does not seem to be so many as to require the agency of yellow fever or any other malignant disease to account for them.

#### NICUESA'S EXPEDITION

After staying about thirty-six hours where he landed to help Ojeda, Nicuesa set out to the west for Veragua, where he intended to settle. It is stated that he had lost no men in the attack on Matarap, yet the detailed distribution of his men given by Oviedo accounts for 50 less than the number (700) which Las Casas and Herrera give as leaving Santo Domingo. According to Oviedo, Nicuesa, with 70 men in a caravel, and Lope de Olano, his Captain General, with 30 men in one of his brigantines, went first, leaving 550 men and the rest of the fleet with Cueto at Puerto de Misas, to wait there until Nicuesa sent the brigantine back to show them the way to the settlement he purposed to make. One feels compelled to accept the 650 thus accounted for in detail as a more probably accurate figure than the round number, "700 men," given by Las Casas and Herrera.

It is not purposed to go into the Iliad of woes of this most unfortunate expedition. After waiting two months, the time set by Nicuesa for his return, Cueto, with all of his part of the expedition, went to look for him. He went as far as the mouth of the Chagres ("*Rio de Lagartos*"), where he stopped the fleet and sent a brigantine, with one of Columbus' old pilots, Pedro de Umbria, "skilled on this coast," to look for Nicuesa. In the meantime, the provisions aboard the ships having spoiled, they were discharged and their boxes used to make huts for the people ashore. Here, then, the first fleet anchored and the bulk of the men remained for the present, and here, too, fourteen men were drowned in the swamping of a boat.



De Umbria, still proceeding westward, found Olano at anchor off the mouth of the river of Belem, in the present Republic of Panama. The two brigantines, Olano's and de Umbria's, went back together to the mouth of the Chagres. There Olano told Cueto that Nicuesa's caravel had been lost with all on board, his own vessel barely escaping wreck. He would give no details, "falling weeping and crying out, 'It kills me to speak of it,'" when questioned. As, in event of Nicuesa's death, the command devolved on Olano, Cueto and his armada accompanied him back to Belem, which he asserted to be Veragua, the place at which Nicuesa intended to settle, although Veragua was, in truth, still some forty or forty-five miles beyond Belem to the west. Here, at Belem, a settlement was made, with Olano for governor, to whom the company—at least some three hundred of them—swore allegiance; and here, and at Santa Maria de Redondo close by, "they remained for eight months" under Olano's government, "suffering grievous hungers and necessities."

Olano's statement concerning Nicuesa was intentionally untrue. There had been a dispute between the pilot aboard his ship (who had been on this coast with Columbus) and Nicuesa as to the position of Veragua. The pilot placed it at Belem, while Nicuesa, relying on a chart made by Columbus, placed it further west. Olano, siding with the pilot, had ceased, in the night, to follow the lantern on the poop of Nicuesa's caravel, which went before to set the course, turned into the harbor at Belem, and allowed Nicuesa to go on alone. Nicuesa waited for Olano a while, then sought him further west, Olano being, of course, to the eastward. Presently, in a river to the west of Belem, Nicuesa lost his own vessel and all material aboard her, including the clothing of his men, who swam ashore "naked," state the historians. They made for themselves jackets of canvas from an old sail that washed ashore, one for Nicuesa as well as the others. He took up his march on foot along the shore, still westward, being helped over the rivers and estuaries by a ship's boat ("*barca*"), saved from the wreck, which, with a crew of four seamen, paralleled his course by sea.

They suffered much, "walking without shoes and nearly naked through salt ponds and marshy places; over ravines and hills and through thickets, living on the shellfish found on the shore and wild things," mainly the buds of certain palms ("*palmitos*"). Finally, crossing an estuary some six miles wide, they reached a small island—not knowing that it was an island. Here their troubles culminated, for the sailors, believing that Olano was behind them, which indeed was the case, went off with the boat to look for him. "They left at night, not asking

Nicuesa, because they believed he would forbid it." They went eastward, reversing their course, and by this act of insubordination undoubtedly saved the lives of the party.

Nicuesa and his men were marooned on this island more than three months, attempts to leave it, by rafts and by a canoe they dug out, being unsuccessful. On this island, which was called "Escudo,"<sup>15</sup> were some trees bearing edible nuts. There was no water on the island save one muddy pond, so brackish that they could scarcely drink it, and—the nuts having been soon exhausted—no food save the shellfish they could pick up on the beach, and wild roots and herbs.

Oviedo states many died of hunger and hardship and that, when rescued, "they were very weak from hunger and in a bad condition and full of worms in their throats and other parts of their bodies from eating certain roots."<sup>16</sup>

The statements of our other historians must be taken with some reserve: that each day saw men die; that they walked about aimlessly, crying out like insane people; that they crawled on all fours, browsing on the herbage, because they were not able to stand upright. Yet their condition must have been deplorable, and, without admitting the hysteria, one has no doubt that a considerable proportion of even very hardy men would die under such conditions, continued for three months or more.

The journey of the small boat to Belem is reported as having been difficult, dangerous, and slow. On its arrival—Cueto was then with Olano—a brigantine with drinking water and such food as they had ("palmitos") was sent for Nicuesa. When he arrived at Belem he put Olano under arrest, in irons, threatening to hang him, and took charge of the expedition.

This was "eight months after the treason of Olano," says Oviedo. Nearly all of this time Nicuesa had spent in his wandering along the shore and on Escudo Island. Nor had things gone well with Olano during this time. He had lost all of his vessels, wrecked on the bars at the mouths of rivers; and the settlement had suffered so much from

<sup>15</sup> The island known now as "Escudo de Veraguas," is about sixty to sixty-five miles along the coast to the west of Belem, but we do not know whether this is the one on which Nicuesa was marooned, "Escudo" (shield) being a rather common name given to small islands at this time.

<sup>16</sup> Possibly the so-called "*gusanos de vacas*" (beef worms, *dermatobia noxialis*), not unfrequently found in the skin of exposed parts of the body in those sleeping out of doors in this country.



famine and privations that he had determined to abandon the venture and return to Santo Domingo, and was then engaged in building a caravel, using the planks of the wrecked vessels for that purpose.

His party had been put on short rations from the beginning. Cueto's armada brought very little food and many to eat it; and attempts to get provisions, from the Indians and otherwise, were in general unsuccessful. There was, indeed, not only scarcity but famine, as is evidenced by their having no food to send their chief save "*palmitos*." Many died. When the men who were at Belem united in begging Nicuesa to spare the life of Olano, they pleaded that he "take no more Christian lives," as 400 of their company had died already. This, one judges, included the deaths from the whole party—those at Belem and those who had been with Nicuesa. A strange plea, however, as the losses of both parties were primarily due to the intentional treachery of Olano.

Nicuesa, when he took command, was dissatisfied with the condition of the colony; there was neither gold nor food, and "the place seemed to be unlucky." He became sick, and as his ill health continued, though he does not seem to have been sick enough to relinquish command, "he became impatient and hard with others and the more so from day to day." He determined to abandon this site and seek a better one to the eastward, on the route he had come. He accordingly pressed the construction of the caravel, begun by Olano, and organized systematic raids on the Indians to get food, both for present needs and for use on the voyage. This led to reprisals by the Indians, who not only attacked his raiding parties, but threatened the settlement "and were not far from finishing the Christians, because our men were too weak from hunger to support their arms." There was the usual "eating of harmful things,"<sup>17</sup> sickness, and mortality.

The expedition remained at Belem while a vessel was sent back to the east to look up a suitable place for settlement, and on its return Nicuesa and as many as there was room for in their transport, followed it. More were left at Belem, says Oviedo, than he took with him. A landing for food was attempted at Porto Bello, in which twenty men were lost, "the Christians being too weak to hold up their arms." At Nombre de Dios, then called "Chuchureyes" by the Indians, and "Puerto de Bastimentos" by the Spaniards, a few miles further on, he landed. "Here,"

<sup>17</sup> As illustrating the extremities to which they were reduced, it is related—though hardly credible—that a raiding party of 30, finding the body of an Indian, "killed by themselves or by others," ate it, although it was slightly decomposed, and that all thirty of them died.

he said, "let us stop, in the name of God (*Paremos aqui en el nombre de Dios*)" and so it was done and Nombre de Dios founded.<sup>18</sup>

His troubles were not over, however, as he had no provisions and no means of getting any save from the Indians. His main source of supply was Porto Bello and its vicinity, "whence food was brought on the backs of men." This is about eight miles distant and, as the writer can testify, even now an extremely rough trail. "They returned to the hungers and adversities of former times," says Oviedo.

Nicuesa began, too, building a wall to defend his new-founded city, "and from this work," state our chroniclers, "he spared none, high or low, sick or weak." He was unable to send for the men left at Belem until some five months later on account of the on-shore winds. There were sickness and death among his men. Herrera and Las Casas state that when the wall was finally finished, 200 more had died, of those left at Belem and of those accompanying Nicuesa to Nombre. This is in addition to the more than 400 deaths that had occurred before Nicuesa left Belem, and it is added that of those brought from Santo Domingo, only 100 were left. Round numbers, however, are rarely reliable.

A vessel from Darien, with provisions, brought by his lieutenant, Colmenares, came in about this time. On this Nicuesa returned to Darien, but he was not allowed to land there, mainly through Balboa's influence, from all accounts. He was placed on a ship with seventeen men "very ill provided," as Oviedo reports, and was heard of no more.

Las Casas puts these happenings as at the end of the year 1510; but if we sum up the intervals of time given by our authors, with even a very conservative estimate for the time not given directly, we can scarcely make the total less than fifteen months since Nicuesa's departure from Puerto de Misas. As that was about the end of November, 1509, his arrival at Darien would be about the end of February, 1511. Indeed, Las Casas elsewhere gives the date of Nicuesa's sailing from Darien—two or three days after his arrival there—as March 1, 1511; and this, one judges, is correct.

Deaths by violence, as reported, account for only a small proportion of Nicuesa's losses: 14 men from Cueto's armada are reported drowned

<sup>18</sup> Possibly Nicuesa, with his half-starved companions, was influenced in his selection of Nombre de Dios by the name and reputation of this "*Puerto de Bastimentos*" (port of provisions), so called by Columbus. Otherwise, the reasons for his decision to "rest here" are not obvious, as Nombre de Dios is scarcely a harbor at all and he had passed by Porto Bello, eighteen miles to the west, and Playa de Damas, both offering more protected harborage.



at the Chagres; 30 died from eating a dead Indian; one was killed by Indians while walking the shore after the wreck of the caravel; and 20 were killed at Porto Bello—in all 65 recorded. Some others, doubtless, were killed in the raids on the Indians at Belem, but no record of these is given. At any rate, there was no such loss by violence as that in Ojeda's expedition from the poisoned arrows of the Caribs. How many were alive when Nicuesa went up to Darien, when our count ceases, one does not know. Herrera says 70; Las Casas, first gives 70 and then 60; and Colmenares, who carried the provisions to Nicuesa and was his lieutenant, says "not more than 200" (Navarrete, vol. 3, p. 386). As the number that left Puerto de Misas was 650, of whom only 65 are reported as having died by violence or accident, we have to account for approximately 395 to 535 deaths from other causes than reported violence. Either figure is a very large proportion, but the conditions, as we have seen, were extremely bad, and they lasted almost continuously for fifteen months. There seems no reason, then, to bring in yellow fever or any malignant sickness of that kind to account for the mortality.

Indeed, even if yellow fever existed at this time on the Caribbean littoral, it is highly improbable that it would have prevailed at Belem, where most of Nicuesa's losses occurred, unless the infection were brought there by Olano's men. The country here was very sparsely populated. Colmenares, (see Navarrete) speaks of Veragua and the coast adjacent to it as being a poor country, not good even for slaves, as the Indians "were only a few fishermen." Olano's difficulty in getting food there witnesses to the same thing; as also the fact that it "had to be carried incredible distances on the shoulders of our men;" and no slaves were reported taken, although slave raids were begun at Nombre de Dios by Nicuesa after he settled there. Had slaves been readily obtainable at Belem, they had doubtless been taken, used as porters, and sold afterwards. So sparsely settled a country could not have remained infected with yellow fever any length of time, and hence was little apt to have been found infected at any particular time. If, however, yellow fever had prevailed at Belem when Olano arrived there, it would have exhausted the human material available within the more than eight months of the time the Spaniards remained. Certainly, this would have happened if yellow fever were the cause of a considerable proportion of the more than 400 deaths reported as having occurred up to Nicuesa's arrival; and as those who reached Nombre would have been immune thereto, the deaths there would be attributable to other causes.

## CHAPTER X

### THE ISTHMUS OF PANAMA: THE COLONY AT DARIEN

#### BALBOA

With the coming of Enciso to Urava, the great losses which the Spaniards had suffered apparently ceased. Immediately after Enciso's arrival at the Gulf of Urava all the men, those who were with Pizarro in his brigantine, who had abandoned the enterprise, and the 150 who came with Enciso, instead of returning to Ojeda's old settlement, moved to the west side of the Gulf of Urava at the mouth of the River of Darien. The Indians here were not Caribs and did not use poisoned arrows. As, in addition, it was "a place where there was gold and much provision," Balboa, who was accredited with having advised this moving, gained sufficient credit to take command of this settlement,<sup>1</sup> well-nigh as great an advantage to it as the escape from the poisoned arrows. Balboa not only made peace with the Indians, but made friends with some of them—especially with Cemaco, the *cacique* in whose territory Darien was situated, and traded for provisions. He began, too, to raise food at Darien, doubtless with the Indian "*naborios*," of whom there were more than twelve hundred when Pedrarias arrived. These friendly relations with the Indians lasted, except on the San Juan (Atrato) River, during the whole of his government, to June 30, 1514. He is credited by Las Casas with having 450 men; but when he took command he had much fewer than this,<sup>2</sup> and in his expeditions used small numbers which could live off the country without oppressing it. In a letter to

<sup>1</sup> As this new settlement was in Nicuesa's concession, not Ojeda's, the latter's assignment of his authority to Pizarro and Enciso was invalid. If the appointment of Vasco Nuñez was, as our chroniclers (Las Casas and Oviedo) say, "*por votos*," it was the first popular election in the New World—the foundation of democracy.

<sup>2</sup> With Pizarro were found less than 70 men; 150 came with Enciso; and somewhat later, with Colmenares, 14 or 24, according to which author we follow; and 17 went back and were lost with Nicuesa. Whether the men left by Nicuesa at Nombre, (variously given as 60, 70, or 200) remained there or came up to Darien, is not stated. Possibly the latter, but Darien and Nombre are over two hundred miles apart, and the current belief is that the present Nombre de Dios dates from Nicuesa's time and has never been abandoned.



the King, quoted by Las Casas (vol. iv, p. 131), he says that "of the 190 men brought out from Darien there could be utilized scarcely 80 because the rest, on account of the hungers and hardships which they suffered, or from being sick or very weak, were not able to aid in anything." After the colony began to get on its feet, men began to come in, as with Serrano, "150 of whom were men of war"—from Santo Domingo, one thinks. There were 600 men at Darien in March, 1511, and Balboa took 800 with him when he set out, September 1, 1513, to discover the Pacific. Yet only 70 men went with him on the overland march to the "peak in Darien," from which he saw the South Sea; and only twenty-six "of the best conditioned and most enduring" marched to this sea itself, when he took possession of it on St. Michael's Day for the King of Spain.

His expeditions, then, making friends with the Indians, getting provisions, and finally going across to the South Sea, were made with small bodies of men. He states, in this same letter, that he had lost no men. Oviedo, however, gives an account of 28 being lost in an attempt to explore the Atrato, and these were probably Balboa's men.

The factors operating to produce this difference in losses between Balboa's and Ojeda's commands were, one thinks:

1. That Balboa moved out of the Carib territory to that of a more peaceful race of Indians, not using poisoned arrows.
2. His making friends with these Indians and arranging to obtain a sufficient supply of food from them.
3. His "instituting agriculture" at Darien.
4. His using, during his expeditions, only a small number of men, who were thus able to live off the country without exciting warlike opposition.
5. The fact that the men he had were used to conditions in the American tropics. The writer would lay as much stress on this last as do Bérenger-Féraud and Finlay, but not as necessarily implying specific immunity to any prevalent infectious disease.

It seems no more necessary, then, to ascribe the small mortality in Balboa's expeditions, as compared with Ojeda's, to the immunity of his men to any specific disease acquired by prior attack than it does to make the same explanation for the comparative safety of the older settlers at Jamestown or of the "*gens des mains*" of the French Antilles.

The settlement at Darien grew and prospered under Balboa's government until 1514. It unquestionably received immigrants, mainly, one presumes, from Santo Domingo. During this time nothing is reported of any special sickness among them. The Indians were peaceful and

food was abundant. When Nicuesa came there from Nombre de Dios, about March 1, 1511, Darien was well settled; there was food, gold was not lacking, and there were "more than 600 men" (Oviedo).

#### PEDRARIAS AND THE "MODORRA" AT DARIEN

On the 30th of June, 1514, Pedro Arias de Avila (Pedrarias Davila) landed at Darien direct from Spain, to supersede Balboa, bringing with him some 2,000 men, the great bulk of them "young hidalgos and courtiers." The policy was now changed. The Indians were raided on all sides for gold and slaves. Friendly *caciques* suffered more than the unfriendly ones, because they were more accessible and took less precautions to escape the Spaniards. Oviedo, himself a soldier, speaks with strong reprobation of the "depopulation of the land" by "this infernal hunt for men." Provisions were no longer obtainable from the Indians, and the cornfields were at this time destroyed by grasshoppers. The food raised at Darien was, naturally, entirely insufficient for the much larger number of men. Pedrarias had brought a large supply of provisions from Spain, which were placed in a storehouse built for them. These provisions were wasted, however, and soon there was a scarcity of food. Later, the storehouse took fire and burned up with its contents, Oviedo stating that it was believed that this was done to conceal the shortage of supplies caused by the peculations of the storekeepers.

The colony was entirely too large to live off the country, especially as the production of food had been diminished by the grasshoppers and by the measures of Pedrarias, nor did the colonists know how to adapt themselves to local food and conditions.

With the famine, as always, was sickness, and while all of our authors ascribe it to the famine, it was, we think, too general for this to have been the sole cause.<sup>3</sup> The sickness—and especially its severity—was, however, unquestionably increased by the famine. Here are the accounts of this episode given by our historians:

First, Oviedo: "And in this infernal chase or hunting were the people engaged some months, in which time . . . prevailed in Darien so great a *modorra* and sicknesses among the Christians and in especial for those who had newly landed from that armada that each day died fifteen or twenty and some days more, and in a little time died more than 500 men, and the most of them for lack of provisions, although the King had them in abundance" (in the storehouse in Darien). Then is told how that

<sup>3</sup> This is also Oviedo's opinion: "an accidental *modorra*," he calls it.



“although men fell dead in the streets from hunger” yet provisions were kept in the storehouse, until the storehouse was burned and all destroyed. Oviedo states that officials did not lack for food. He does not say whether they escaped the sicknesses or not.

He continues: “Since as the deaths and sicknesses were many and the hunger intolerable, many who were in that armada fled from the land. Some returned to Spain and others passed to this our island of Hispaniola, to Cuba and to Jamaica, and to San Juan; and in the space of seven or eight months<sup>4</sup> those who had fled and those who had died were more than those who remained in this land.”

Herrera gives: “Whilst he” (Pedrarias) “was arranging and preparing to send the people to the three settlements,” which he was planning, “they began to waste the provisions of the fleet, which had already been spoiled by the sea, and rations began to be scant; on which account and because the place was sickly where they were settled, which were marshes and low and shady places, and because of the differences of the airs . . . . the new men, who had come with Pedrarias, began to sicken and to die. And he (Pedrarias) did not escape, and his sickness having become very severe he went out of Darien” by his physician’s advice, to a healthier place. “And when all the rations of the King were finished, the calamity of hunger increased to such a degree that many cavaliers died begging bread, who had left their estates mortgaged in Castile, and others who gave a cloak of crimson silk and other rich vestments for one pound of bread of maize or of cassava or hard bread of Castile. A cavalier, one of the principal ones who had come with Pedrarias, went out into the street crying out that he was perishing for bread, and in front of all the people, falling on the ground, his soul departed. Never does it appear that a like thing was seen; that people so attired in silk and in brocade, and who were wealthy, should fall at each step dead of hunger. Others were going out in the fields grazing and eating the herbs and roots which they found most tender, as if they were beasts.”

Herrera says that they were buried several in one grave, “and to conclude, in one month died 700 men of hunger and of *modorra*.” He blames Pedrarias severely for the confusion, disorder, and profiteering that prevailed.

Las Casas adds nothing to the above, his account agreeing almost verbatim with that of Herrera, except that he does not give the number of deaths.

<sup>4</sup> The implication here is that this condition lasted seven or eight months.

No description is given of this sickness, unless that it was called a "*modorra*" (drowsiness or stupor) may be considered in some sort as a description. This designation was habitually used by Spanish writers of this time for typhus and typhoid fevers, but obviously it does not refer here to typhus, for no lice-borne infection could have propagated itself in the climate of Darien.<sup>5</sup> The use of the term, "*modorra*," may not have been confined, especially by laymen, to the fevers above mentioned, yet the writer knows of no instance in which it was used as the designation of any epidemic of what was certainly yellow fever, as that of 1648 in Yucatan, or of 1649 in Havana. And it would not naturally be so used, for the characteristics of yellow fever are restlessness and delirium rather than a drowsiness and stupor, even the short final coma, when it occurs, usually ending in a convulsion.

This "*modorra*," however, in the conditions under which it occurred, is more like yellow fever than anything that we have found in the New World prior to 1648—a generalized sickness of high mortality among a body of newly arrived immigrants in a tropical climate. This is what would occur with yellow fever in an endemic center. The only thing in direct opposition to it is Oviedo's implication that the epidemic lasted seven or eight months. This, of course, had been impossible with yellow fever of the intensity reported among so small a body of men.<sup>6</sup> This long duration is only an implication, however, not a direct statement. On the other hand, famine was severe; and though the mortality was high (one-fourth of the total number),<sup>7</sup> it was not higher than that occurring in other settlements of Europeans in America where there is no suspicion of yellow fever; as at Plymouth in the first winter, at Jamestown, and in Laudonnière's colony, etc. Moreover, Pedrarias' men—hidalgos and men of the court—were peculiarly ill-fitted for frontier conditions. The writer, too, has seen practically 100 per cent of Spaniards (*Gallegos*), new-come to this same Isthmus, sent to hospital with malaria within eight weeks of arrival.<sup>8</sup>

<sup>5</sup> The relapsing fever of the tropics, the tick-borne *spirochetosis*, could have done so had the causative organism been present and this would have merited the name "*modorra*." There is no evidence that this occurred, however.

<sup>6</sup> Malaria in that time would have shown an increase in mortality by its recurrent attacks.

<sup>7</sup> The number of deaths (500) given by Oviedo is accepted as against the 700 of Herrera. Oviedo was on the ground and had every opportunity of knowing. If Herrera, in his reports from the *tierra firme*, had a record of 700 deaths, it might well be a question what districts or what length of time it covered.

<sup>8</sup> This was in 1906 in the Culebra District, at Lirio, as I recall, and, unlike Darien, on high land. Following the lead of Pedrarias with Darien, we also "depopu-



While, then, it is not possible to say that this "*modorra*" was *not* yellow fever, the evidence is not sufficient to establish it as such or even to render this very probable.

#### PEDRARIAS THE YOUNGER

Finlay notes another sickness among the Spaniards who came with Pedrarias. He sent his nephew, of the same name as himself, with 200 men to the Cenú River, about 100 miles from Darien. They embarked in small boats to row up the river. The Cenú was swift, and poling against the current was hard. The sun was very hot, the mosquitoes numerous, "and they commenced to fall sick and to die." Young Pedrarias, not finding any place suitable for settlement, returned to Darien, "having lost one-half of his party." This is from Las Casas (*op. cit.*, tom. iv, p. 180).

There is no statement as to the length of time young Pedrarias was on this expedition. He apparently made no stop of any length of time ashore. It would be well-nigh impossible for an epidemic of yellow fever, carrying off 50 per cent of his force, to occur in so small a moving body of men. On the other hand, malaria quite probably had been introduced into this region by the Spanish communication with it from the time of Bastidas, Guerra, and de la Cosa, say from 1502; and even accepting this account as true, as Finlay does, it could scarcely be considered yellow fever.

Herrera mentions this same expedition, but gives young Pedrarias 400 men. He says nothing about sickness, but that "men were killed in encounters with the Indians"—fifteen in one attempt to land. This was Carib country. Enciso also took an expedition to the same region and was roughly handled by the Caribs. Oviedo does not mention the expedition of young Pedrarias, although he gives a long account of the expeditions of the subordinates of Pedrarias, including that of Enciso. One wonders where Las Casas got his data, and except that Finlay quotes this, I had not mentioned it (Finlay, 1912, p. 119).

---

lated" this village and moved our own men, as they emerged from hospital, to Las Cascadas. Our deaths, indeed, were nothing like the proportion as given at Darien, but then we had an abundance of food for both sick and well, good hospitals, and *quinine*. Had the same conditions prevailed as at Darien, one has an idea that the death rate would have been very high.

## CHAPTER XI

### GUADELOUPE: THE COUP DE BARRE, 1635

#### AUTHORITIES

For the reasons which have been given in Chapter VII and elsewhere, the writer accepts the epidemic of 1648 in Yucatan as unquestionably yellow fever, and as being the first epidemic in the New World that can be so identified with certainty. He thinks, then, that this year, 1648, may be taken as marking the division between what Chaillé (1882) has designated as "traditional" and "historical" yellow fever. Certain it is that after 1648 reports of epidemics of what we believe—and in some instances are sure—was yellow fever become common, and are well enough authenticated to be considered, as Chaillé characterizes them, "historical." However, Chaillé himself, accepting as undoubted yellow fever the "*coup de barre*" which occurred in Guadeloupe in 1635, designated that year as dividing the "historical" from the "traditional." Let us then consider whether the "*coup de barre*" is identified as yellow fever.

As to the nature of the "*coup de barre*" from which the early colonists of Guadeloupe suffered in 1635 and later, let us examine (1) the conditions under which it occurred and (2) such description of it, clinical and otherwise, as we have. Our authorities are Père du Tertre and Père Breton and, negatively, Père Bouton, all members of the Society of Jesus, and all missionaries to the French colonies of the West Indies and Guiana.

Père du Tertre was evidently a highly cultivated man, intelligent, well informed, and used to the great world of Europe. His *Histoire Generale des Antilles habitées par les François* (1667–71), in four volumes, covers from the settlement of St. Christophe, in 1627, to 1667 and beyond. It is full of interesting detail, not all of it important for our purpose. He did not come to the islands until 1640, and was thus not an eye-witness of conditions during the first five years of the settlement of Guadeloupe.

Père Breton was also an intelligent and cultivated man. He came to Guadeloupe with the first colonists in 1635, and his account is contained in two dictionaries—one Carib-French, (1665), and one French-Carib (1666). No one save an industrious and most intelligent and observing



man would have written them—the Carib-French especially. This is really a brief history of the islands and of their inhabitants and products as they were when he arrived and soon afterwards. He was an eye-witness to the “*coup de barre*” in the beginning and to the “*disette*” (scarcity of food, or famine) which preceded it. Indeed, his clinical description of the former—a strange thing to find in a dictionary—is so like that of du Tertre, even in the turns of expression, that one feels quite sure that the latter was copied from it and thus, also, is the record of an eye-witness, but of Breton, not du Tertre.

#### SETTLEMENT OF GUADELOUPE

Guadeloupe was settled by de l'Olive and du Plessis under the auspices of a French Colonization Company. St. Christophe (St. Kitts) had been settled in 1627 by two adventurers, d'Enambuc and Warner, the one French, the other English, who had combined against the Spaniards. In spite of some disagreements and even small wars between the two races, it was, in 1635, an established and flourishing colony, and later became a resort of the filibusters of both nations.

Du Plessis and de l'Olive left Dieppe on May 25, 1635, with two vessels, having aboard 550 colonists and four priests, among whom were Pères Breton and Pelleprat. They reached Martinique June 25, but decided not to settle there on account of its being too mountainous, according to our historian; on account of the multitude of serpents, says Père Pelleprat. They reached Guadeloupe on June 28, choosing, says du Tertre, a bad place for settlement, “the land being red and better suited for making bricks than to grow plants.”

The troubles of the colonists had begun aboard ship. They were both ill and scantily provisioned, and much of what they had spoiled en route. So little cider had been provided that it was “extended” by adding 50 per cent seawater to it, “which caused an unbelievable alteration in all the passengers . . . so that some died as soon as they landed.” “All were half dead when they landed.” They were said to be “*pauvres engagez*,” utterly unsuited for pioneer life, and too many to live off the country. Nor had they leaders of sufficient force and experience properly to guide them. They had brought neither “potatoes, manioc, peas, nor beans for planting.” They had to send to St. Christophe to get these things for seed. They depended solely on France for food, of which they had brought only two months' supply. It was necessary to put them on rations from the beginning: at first one pound of “*paste*” (one presumes the wet flour they brought with them), soon reduced to five ounces. There were turtles on the beach, but eat-

ing them without bread caused sickness (diarrhea and dysentery) of which some died, and turtles were for a time forbidden.

#### FAMINE

A "horrible" famine then afflicted the colony two months after its establishment . . . . "you would see hungry men grazing herbage like beasts and eating their own excrement." "Seeing themselves prevented from procuring food to satisfy this hunger, they exposed themselves voluntarily to extreme severity of punishment, preferring rather to end their misery by the hands of the executioner than to drag out longer a life which famine had rendered more cruel than death." Some little food was taken from the Indians on the island. The Indians themselves, however, were living a hand-to-mouth existence, and the Indian war which followed added to the distress of the colonists, as they could no longer hunt or go into the woods for roots, fruits, etc., and had to stand regular watches against surprises. They ate the few famished dogs they had, and even the bodies of their dead. The good Father gives an instance (and says that it was not the only one) in which the trenches in which the dead were buried were opened, and the bodies eaten. One man who had been branded twice on the shoulder, when saved from the gallows by the intervention of Père Breton, deliberately repeated his offence, of theft, "preferring to be promptly hung rather than to live longer exposed to the insupportable rigors of this hunger" (Du Tertre, vol. 1, p. 75 *et seq.*).

Of this famine, Père Breton, who was with it from the beginning, gives this (*Dictionnaire Caraïbe-François*, 1665, p. 226-7): "*Eúmijn—famine . . . disette.*" He states that when they were at war with the Caribs they dared not go into the woods or along the coast to look for food, for the Caribs killed as many as they could find, and "the scarcity (*disette*) of bread and of water made them" (the colonists) "waste away in such sort that they were more yellow than quinces and more dry than Brazil wood, having only skin and bones: in taking tobacco, in speaking, and walking, they fell in agony without other malady than pure necessity and feebleness. We buried them four and four in one ditch, which we had enough trouble to have made, not finding anyone" (strong enough) "for that purpose, nor to carry the dead. When we exhorted the sick men to confess themselves, they gave no other answer than 'bread! bread!'" It was evidently a severe famine.

Again, du Tertre (*loc. cit.*, p. 81): "This famine, which lasted nearly



five years<sup>1</sup> . . . was followed by a mortality nearly general to which, besides the famine, two things contributed particularly." The first was "a certain malady which is commonly called *le coup de Barre* in these islands. It causes ordinarily to those who are taken (*surpris*) with it, a very violent headache, accompanied by a beating of the arteries of the temples and by a great difficulty of breathing with a weakness and a pain in the thighs, as if one had been struck blows with a stick (*coups de Barre*) which has given reason for the name imposed on it. It attacks ordinarily those who clear up the lands of these islands, on account of the venemous vapors which they exhale.

"The cruelty of the Commandants, who presided over the work, was the other cause of death of the greater part of the colony. For although the poor *engagez*, whether pledged to the gentlemen of the Company or to the merchants of Dieppe, were extraordinarily enfeebled by misery and hunger, they were treated worse than slaves, and they were forced to the work only by blows of sticks and of halberd shafts; insomuch that some of them, who had been captives in Barbary, cursed the day when they left that place."

Du Tertre especially notes that no ration (at one time five ounces of "paste") was issued to the colonists until they had worked to noon, which he, and apparently they, considered a hardship. This was both a sanitary and an economic error.

It seems proper here to give also the description of Père Breton, who was an eye-witness to the things he relates; du Tertre was not an eye-witness to anything occurring prior to 1640. Breton's dictionary (1665, p. 276) gives the following:

"*Ibómanhatina iepoulicáatina*, in the commencement of the colony in the isle of Guadeloupe, partly by famine, partly because as in measure we cut down the wood, the land threw out all its venom, we were taken with a beating of the head within the temples, with a short breath

<sup>1</sup> It is not possible that the famine lasted from 1635 to 1640 with the severity depicted above. In a less time than this all had died. The "*disette*" must have been merely a "scarcity" after the early part of the five years. Nor do I think that the words "was followed by" means that the events to be related began as soon as the famine was past (according to this in 1640), but that the famine came first and the events the historian is about to relate began *after it began*. Père Bouton (1640, pp. 88-95), who visited Martinique in 1640, gives a section to the sicknesses of "these Islands" (using the plural) but makes no mention of the "*coup de barre*," although he does mention intermittent fevers, yaws, chigoes, etc., as existing in them. Nor does he note any lack of provisions. Martinique, at that time, had 1,000 Frenchmen and "would have had 2,000 were it not for the fear of serpents"—of which there are an abundance. The *fer-de-lance* exists in Martinique and, I think, in no other of the Little Antilles.

and with a great weakness of the thighs, so that we were as if someone had given us a blow with a stick (*coup de barre*), and indeed, a man in order to express this illness would say, 'I have the *coup de barre*.' It must have been that the savages well knew this sickness: for they name it as it is at the head of this article" ("*iepoulicáatina*").

#### THE COUP DE BARRE

This statement that the "*coup de barre*" was an indigenous disease among the Indians and well known to them before the advent of Europeans—its occurrence among the French was 142 years after the settlement of Isabela—is quoted and insisted on by many writers. That the Caribs of Guadeloupe called this malady, of which Père Breton is writing, "*iepoulicáatina*" and that "*iepoulicáatina*" is literally "*coup de barre*" in their tongue, is directly stated by the good Father and may be accepted. However, his deduction from these premises, that these Indians "knew well this malady," in the sense of having previous knowledge of it, seems a *non sequitur*. We have seen how quickly the Mexicans and Mayas adopted descriptive names for smallpox, of which they certainly had no knowledge before the Spaniards came, the Mexican name (*huey çahuatl*: great pox) stressing its appearance; while the Maya name (*pom kakil*: gum copal fire) described its sensation. That the Caribs should, *of themselves*, have selected the literal equivalent of the name which the French, of their own initiative, gave to the so-called "*coup de barre*" when it appeared among them, seems most improbable; and when this name, in both instances, is not so much the description of a symptom as the designation of an action which might cause the symptom, the chance of both French and Caribs selecting this same name independently of each other becomes extremely small. On the other hand, if this sickness was hitherto unknown to the Caribs, they would naturally, almost inevitably, adopt for its name the one the French gave it, translated into their own tongue. The fact that "*iepoulicáatina*" is literally "*coup de barre*" is, if the latter name was given to this sickness by the French independently of the Caribs (as is implied by Breton's narrative), evidence that the Caribs got their name for it from the French and that they were *not* previously familiar with this sickness, else they had retained their own name for it.

What this "*coup de barre*" was, the writer does not know. The description of it is not especially like yellow fever, less so indeed than it is like that of a number of other acute diseases which might occur in the tropics; and indeed it is only the use of the word "*surpris*," translated



“taken,” that makes one limit the “*coup de barre*” to an *acute* disease, and this may be an error.

#### SIGNIFICANCE OF THE DATA

No figures are given of the number of deaths. It is implied that they were many, especially in Breton's account. Yet, receiving few reinforcements from France until 1640 or later, and these being only colonists, with practically no provisions—“an onus, not a help,” says du Tertre—they held their own in war against the Caribs of this island and those of St. Lucia, Dominica, St. Vincent's, and Barbuda as well, finally cleared land enough to raise their own food, and established the colony. In 1664, it was a flourishing colony—and with apparently as many scandals afloat as St. Germaine itself. Enough survived, then, for these purposes. Note, too, that of the four priests, none died at this time. In the “*peste*” of 1648 several died at Martinique and St. Christophe.

Making what allowance one feels reasonable for exaggeration, we must admit that there was a severe famine and that the living conditions were extremely bad. We would expect a heavy mortality among 550 “*pauvres engagez*” left in the woods on a tropical island with two months' provisions. It had been a miracle had it not occurred. Had yellow fever of ordinary intensity attacked them, the case mortality had been 100 per cent—and all were susceptible. The writer agrees with Finlay (1912, p. 141) that this “*coup de barre*” could not have been yellow fever, on account of the low mortality; and with Cornilliac (1886, p. 304), that it was a disease differing in its characteristics from that occurring in 1648, because: (1) the descriptions differ; and (2) the “*peste*,” introduced into Guadeloupe in 1648, was designated by du Tertre as a disease “hitherto unknown since the occupation of these islands by the French.” The “*peste*” of 1648, we feel reasonably sure, was yellow fever, from its description, the epidemiology given, and its high mortality.

#### DESCRIPTION BY DU TERTRE

Concerning the nature of this “*coup de barre*,” the quotation given from du Tertre is the one and (save by Cornilliac), I think, the only one given by writers discussing this subject. There is, however, another note by du Tertre on this malady, or it may be on a different one called by the same name, which the writer has never seen quoted anywhere. In his *Histoire Generale des Antilles* (vol. 2, p. 477 *et seq.*) we find: “Of the Sicknesses to Which the Inhabitants of our Islands are Subject.” This was written when the colony was well established and flourishing, long after the account first quoted. After noting the occurrence here,

as in France, of intermittent and continued fevers, pleurisies, colics, and dysenteries, he says the islanders "are also subject to certain illnesses which they call stomach disorders (*maux d'estomac*) and hydropisies which disconcert all the surgeons and cause two thirds of all the deaths on the islands, but particularly of the poor *engagez*." Ascribing their sickness to indiscreet habits, he says of those newly arrived from France that "they drink water as cold as ice, and, wet with the sweat of the day's toil, they lie uncovered in the coolness of the night. . . . Then they are taken (*surpris*) with a debility, with lassitude and with troubles (*maux*) of the heart, which are followed by slow fevers and above all by the sickness which they call "*coup de barre*," which is no other than a pain that takes them in the middle of the thighs, which prevents them from moving, and all this is accompanied by a raging thirst, which brings after itself the *hydropsy*. A yellow bile spreads itself sometimes over all of the body, and sadness and melancholy usually taking possession of their spirits, it finishes them and puts them in the tomb. . . . Our surgeons order them to walk about and take exercise, but the difficulty of breathing prevents this."

He mentions, also, as another disease peculiar to the islands, "a species of paralysis, or swelling of the nerves and tendons, caused by a viscous phlegm . . . whence it happens that the members, especially the arms and the legs, remain destitute of strength and sometimes deformed."

This is a pretty fair picture of beri-beri—not entirely accurate, of course—or, leaving out paralysis and the suddenness of attack apparently implied by the word "*surpris*," of the dropsy and debility from any profound anemia, as from hookworm or malaria—the former especially. That it occurred especially among the *engagez* would be characteristic of both.

Whether the "*coup de barre*" here described was the same disease as that called by the same name in the early years of the colony, is a question. The writer doubts it. The calling of two diseases by a common name, and the same disease by two names, has wrought much confusion in tropical medicine.

It seems strange that this "*coup de barre*" has been so generally accepted as certainly yellow fever; especially so when that opinion has generally been based solely on the account of it by du Tertre; Breton's books with his expression "*plus iaunes que des coins*" having generally escaped notice. This, indeed, has added little to justify such a conclusion, as it is reported to have occurred from starvation, without any sickness.

Indeed, considering all conditions, not only is it not certain, or even especially probable, that the "*coup de barre*" was yellow fever, but it seems highly improbable that it was.



## CHAPTER XII

### YELLOW FEVER IN AMERICA AFTER 1648

#### CUBA: FREEDOM FROM YELLOW FEVER BEFORE 1648

We have said that reports of epidemics of what we are quite certain was yellow fever became not uncommon after 1648. The history of Cuba well illustrates this. Finlay, to my mind the most logical and most erudite of the advocates for the American origin of yellow fever, writes (1912, p. 122):

“From 1511, in which year Velásquez came with 300 residents of Sto. Domingo to settle the Island of Cuba, to the year 1648 or 1649, in which its population increased to a little less than 30,000 inhabitants, in spite of having an important commerce and that the generality of the vessels which sailed from Nombre de Dios and Vera Cruz for the Peninsula or for Sto. Domingo, touched at Habana, there was no notice that any of the inhabitants had suffered on its soil from the grave pestilences which year after year were wont to afflict those who went to the ports of the neighboring continent mentioned above. The principal criterion which we have to recognize that a locality has not suffered before from yellow fever<sup>1</sup> . . . is the aptitude which the generality of the population in such localities manifest to contract the disease when any epidemic of yellow fever comes to invade their territory for the first time.<sup>2</sup> With its help we see corroborated our assertion that in the first 138 years of the occupation of this island of Cuba by the European race there has not been manifested in it the pestilence which immediately spread through all the Island, from the year 1649 to 1655, and of whose nature there is no plausible reason to doubt that it was the same yellow fever which subsequently has come to establish here its kingdom.”

Again (*ibid.*, p. 125) “The Island of Cuba, by a singular exception, in spite of its proximity to foci of infection and due, without doubt, to the

<sup>1</sup> In place of “before” he might have written “before or for a long time,” so as to allow a sufficient population susceptible to yellow fever to grow up or come in. The general opinion, however, at that time, 1884, was that a place once infected with yellow fever tended to remain infected unless freed by cold weather or some similar agency.

<sup>2</sup> Finlay credits this idea to Chaillé of New Orleans, also an able yellow fever epidemiologist.

benignity of its climate, remained free from the invasion of yellow fever during the 138 years of its settlement by the Spaniards until 1649, in which the pestilence was introduced from the neighboring continent<sup>3</sup> and extended over all of the Island, destroying one-third of its inhabitants the first year and continuing its ravages to 1655."

Considering our knowledge of how readily yellow fever is carried by vessels, especially sailing vessels, a reasonable explanation of the freedom of Havana from this disease for 138 years, in spite of a large susceptible population and a large commerce for which it was a port of call, would be either that Havana was "uninfectible territory"—that is, not infested with *aegypti*; or that the vessels calling there did not, in general, come from ports in which yellow fever prevailed. Both may have been true in the beginning, for the Little Antilles and Yucatan seem, like Havana, to have escaped yellow fever for many years.<sup>4</sup>

#### HAVANA: THE EPIDEMIC OF 1648 OR 1649

For the introduction of yellow fever into Havana, of which Finlay speaks, we quote from Pezuela's *Diccionario Geográfico, Estadístico, Histórico de la Isla de Cuba* (Madrid, 1863-66, tom. 3, p. 23): "1648. *Peste* of putrid fevers in Havana and in the fleet of Don Juan Pujadas, stationed in the port almost all the summer. Three assessors of government died, named successively" (*i.e.* one appointed as soon as his predecessor died), "an *alcalde*, many functionaries, a third part of the garrison and of civilians in the neighborhood, and an even greater proportional number of the crews and passengers of the vessels."

<sup>3</sup> It sounds strange for Dr. Finlay to ascribe freedom from yellow fever to the benignity of climate. This paper (republished in 1912) was read in November, 1884, about three years after he had promulgated the doctrine of the conveyance of yellow fever by the mosquito (*Aedes (Stegomyia) aegypti*). Possibly he was less firmly convinced of this at that time than, from further investigation, he became afterwards, although I think his belief in it was *absolute* from the beginning—certainly when I knew him in 1899 and 1900 it was so. I think it was not even strengthened by Reed's demonstration of its truth.

<sup>4</sup> It is worth noting that Finlay, though an earnest advocate of the American origin of yellow fever, believed that it existed in none of the Great Antilles (Porto Rico, Santo Domingo, Cuba, or Jamaica) when they were first visited by Europeans, which, indeed, the early history of these islands confirms. He believed, however, that it was brought to Santo Domingo by Caribs, presumably from the mainland, just prior to the great battle of Vega Real, March 24, 1495, after which, following a famine, a great mortality occurred among both the Spaniards and the Indians.



There is no entry in this book, *Pezuela's Diccionario*, for 1649; the next is 1653 and about a political change; but in the *Historia de la Isla de Cuba*, by the same author (Pezuela, 1868-78, *tom. 2*, pp. 106-109), we find the following: "In the spring of 1649, supervened to terrify it" (the country) "a horrible epidemic. From that of smallpox, which decimated the new-born (*nacientes*) pueblos . . . . at the beginning of the sixteenth century, there had not been known other contagions and sicknesses than those inherent to its warm climate and the malignant fevers of the summer of 1620. The records of Governor Villalba neither detailed, nor even explained the symptoms of the sickness which then prevailed in many coastwise settlements of the continent and which was supposedly introduced into Havana by vessels from Cartagena and Portobello . . . . A third part of its population was devoured from May to October by a species of putrid fever which carried off those attacked in three days. The therapy, tried gropingly (*á tientas*) by some experimenting physicians (*facultativos tanteadores*) . . . . against an unknown sickness, aggravated instead of curing it . . . . Villalba" (the governor) . . . "fell sick also in August." He recovered, but his temporary successor, the auditor, Molina, died, as also the three licentiates (names given) "who, on the death of Molina, successively took his place; an *alcalde* and many functionaries, a third part of the garrison and of civilians in the neighborhood, and an even greater number of the crews and passengers of the squadron. Its general, Don Juan Pujadas, not to change his orders, made the error of stationing it in the infected port." The losses which the garrison suffered by the epidemic were replaced in 1650 by officers and soldiers from Vera Cruz and Cadiz. *Pezuela's Historia*, from which this is taken, mentions no sickness in 1648, the year which his *Diccionario* gives as the date of the epidemic.

Were there one or two epidemics? It does not seem reasonable that the same author, in writing two different accounts of such an event, would make an error of a year in its date. It might be that the infection, and if yellow fever, its vector, was first in the squadron of Pujadas in 1648, and that *aegypti* did not sufficiently infest the town to allow a general epidemic therein except in the neighborhood of the *flota* until the next year. Yet *Pezuela* wrote about 220 years after the event and, unless we knew the sources of his information, (apparently the records of Villalba), we cannot deny the possibility of an error in one of his dates. If one notes the parallelism of the two accounts and the similarity—even identity—of phraseology, the most probable conclusion seems to be that they were copied from the same original, or that the manuscripts

from which they were taken had themselves been copied from the same original, and that there was *only one epidemic*. Noting the date (1650) at which the levies are recorded as sent to fill the losses of the garrison, one would judge that the epidemic was in 1649, rather than 1648. Also, the *Historia* (tom. ii, p. 112) states that the fever in Santiago de Cuba in 1653 was *three* years after the Havana epidemic. Again, if this fever was introduced from the continent, its most probable source, directly and indirectly, had been the epidemic which began in June, 1648, at Campeche. Indeed, Pezuela's *Historia* says (foot note, tom. ii, p. 107): "This *peste* of putrid fevers had afflicted Vera Cruz . . . and other pueblos of New Spain" (Mexico) "the preceding summer." This could only refer to our Yucatan epidemic of 1648; hence 1649 seems to be the most probable date for the occurrence of this (single) epidemic in Havana.

One notes, however, the same abundance of buccaneers about Havana in this and the year immediately preceding as were about Campeche, only in greater number; and, as we thought was probable for Campeche, they may have been the introducers of the infection here, directly or indirectly through the *flota* of Pujadas.

Let us note three statements in the account of this "*epidémia*:" (1) Pezuela corroborates Finlay's assertion that except for smallpox, Cuba had been always notably free from contagions and sicknesses; (2) he states that this sickness was unknown to the physicians, and (3) he alludes to an epidemic, or at least a number, of malignant fevers in the summer of 1620.

#### HAVANA: MALIGNANT FEVERS OF 1620

Could this sickness of 1620 have been yellow fever? This date is sometimes given as the earliest introduction of yellow fever into Havana, although the writer thinks 1649 is the one generally accepted. Of the year 1620, the *Diccionario* of Pezuela, which we have just quoted, says only: "1620. With the name of '*Armada de Barlovento*' some war vessels destined for the pursuit of pirates in the archipelago of the Antilles began to anchor frequently in the harbor." His *Historia* (tom. ii, p. 21) however, gives: "From June an epidemic of pernicious fevers decimated Havana, which lasted to November and carried away many victims from the shipping (*flota*) also." This "*flota*" was called "*la Armadilla*"—the Little Armada—and was presumably the same mentioned in the *Diccionario* as "*de Barlovento*." At any rate, it had been



chasing pirates off the coast of Venezuela and about Tortuga close to Santo Domingo.

Through the courtesy of Dr. Beato of Havana, by personal communication, which is most gratefully acknowledged, the writer is able to present the mortuary statistics for Havana for this time:

<i>Defunciones</i>	
Año 1619.....	40
Año 1620.....	64
Año 1621.....	72
Año 1622.....	56
Año 1623.....	49
Año 1624.....	38

A larger number of years had been better; from these one cannot determine the normal number of deaths per year. The mortality in 1620 is raised 60 per cent above that in 1619, but it is still higher in 1621, and is nearly as high in 1622 as in 1620. It looks rather as if the number of deaths had been increased for these three years, 1620, 1621, and 1622, which average 64 deaths, while the other three average only 42. Inability to determine the average annual mortality blocks us; but influenza or malarial fevers would fit this picture better than yellow fever, which, indeed, does not suit. To the writer the picture seems to be that of malarial fevers, severe in 1620 and 1621, and persisting in milder form for some years thereafter, the excess of deaths after 1621 being due to recurrent attacks in individuals weakened by previous attacks and chronic malaria.

The population of Havana at this time, Dr. Beato says, "was 4,082, according to the report of the year 1620 sent to His Majesty by the Bishop of the Island, Fray Alonso Enriquez de Almendares." At any rate, then, the mortality of 1620 was a little less than 16 per thousand, even without subtracting the deaths among the garrison and the crews of the *Armadilla*, among whom the fever is said to have had "many victims." This is low enough for an ordinary healthful year, although apparently greater than normal for Havana at that time, since it is 60 per cent greater than that of the preceding year. Dr. Beato also says that this fever did not excite enough alarm "for the authorities to call for rogations or processions." It was not an *epidemic* of yellow fever. If it was yellow fever—and there seems no special reason to believe that it was—the vectors were not at that time sufficiently prevalent in Havana to make an epidemic.

## CUBA: EXTENSION OF YELLOW FEVER 1649-55

There is no detailed description of the "*epidémia*" of 1649, but considering (1) its high mortality; (2) its generally rapid progress to death; (3) its prevalence aboard vessels and in Havana, and afterwards in other Cuban towns; (4) its communicability; (5) the exclusion of typhus and all louse-borne diseases on account of the climate; and (6) the known presence of yellow fever in 1648 in Yucatan and in the French Antilles, with which Havana was in communication, directly by Spanish vessels or indirectly by the buccaneers, the writer thinks that this sickness can be identified with reasonable certainty as yellow fever. Not, it is true, with the absolute assurance which we have for the epidemic of 1648 in Yucatan, or that of 1686 in Bahia.

Finlay (1912, p. 122) states that this "*pestilencia*" immediately extended all over the island from the year 1649 to 1655 and that it lasted to 1655 in a recognizable form and much longer in the larval form (the "*fièvre bilieuse inflammatoire*" of Bérenger-Féraud); and we again quote from Pezuela's *Historia* (tom. ii, p. 112): "In July and August of this year" (1653) "the same fevers afflicted Santiago and Bayamo as the capital had suffered three years ago, with equal havoc as in Havana." This implies that no epidemic of yellow fever had visited these places in 1649—else there had not been sufficient susceptible people to support one in 1653. That the "*peste*" was at least quite generally feared over the island in this year is shown by the measures which Pezuela says were taken to prevent its introduction into the Cuban towns: "Xelder" (the governor) "in order to preserve the capital from a new scourge, agreed with the *Cabildo*" (of Havana) "that all communication with the other towns of the island should be cut off; and they induced the authorities of Trinidad, Sancti Spiritus, Puerto Principe, Baracoa, and Remedios to imitate the same example."

## ENDEMIC YELLOW FEVER IN HAVANA

Yellow fever finally disappeared from Cuba after 1655 and was reintroduced in 1761 from Vera Cruz, with the prisoners sent to help build Cabañas, when it ravaged the British forces under Pocock and Albermarle, who that year besieged and captured the city. From that date it was permanently endemic in Havana until 1901, when, as is well known, it was banished by Gorgas, using sanitary measures based on the knowledge of its conveyance by the mosquito, *Aedes* (*Stegomyia*) *aegypti*—the first place in which this was ever done.



Havana well illustrates that endemic yellow fever does not prevent a town's being prosperous. It was a prosperous and beautiful city during the latter part of the nineteenth century before the last Cuban revolution. Nor was the death rate from yellow fever among the civil population large—with the military it varied with the number of soldiers sent in from Spain. The writer, in 1900, made a careful study of the mortality statistics, which were beautifully kept (Carter, 1900-b). From April 1, 1884, to April 1, 1894, ten normal years not affected by the war, the average number of deaths from yellow fever recorded among civilians was 210.4 per annum; the total mortality from all causes during the same period averaged 6,677 per annum; that from tuberculosis 1,492 per annum for the last five years of this period.

This study was confined to civilians, because the number of deaths from yellow fever among the military forces showed extremely wide fluctuations from year to year, due to their varying number and their introduction *en masse*, direct from Spain, at irregular intervals of time, so that no significant annual average of deaths was determinable. "Civilians" included all in Havana—Spaniards, Cubans, and foreigners—except soldiers and sailors.

The population of Havana during this time is not recorded, but its size would have influenced very little the number of deaths reported as yellow fever, since deaths of persons native to Havana or long resident therein were rarely—the writer thinks never—so reported. Unquestionably, a certain number, and it may be in epidemic years a considerable number, of young children native to Havana did die of yellow fever, as Guiteras (1888) has shown; but with the doctrine of "Creole immunity by place of birth," at that time held by the laity of Havana and by some of its physicians, such deaths would not have been reported as yellow fever. There were, then, more deaths from yellow fever in Havana than were recorded, but, yellow fever being generally a mild disease among young children, probably no great number.

More important than the size of the population of Havana, as affecting the number of deaths recorded as yellow fever, was the amount of immigration. The records showed that during the ten years studied this averaged from 15,000 to 20,000 per annum, practically all from Spain or Spanish overseas possessions—the Balearic and Canary Islands. These immigrants, of course, furnished nearly all of the deaths reported as yellow fever. A considerable number of them, however, especially those from the Canaries, did not remain in Havana.

Obviously, then, even with a large immigration of susceptible people,

yellow fever was a small factor in the total mortality of Havana, and while its presence was a handicap to the city's trade, yet this flourished and prospered in spite of the disease.

It may be of interest to sanitarians to state here that a rather careful estimate made at this time seemed to show that only about one out of ten or twelve of these Spanish immigrants (*Peninsulares*) who remained in Havana five full years were recorded as having suffered from yellow fever during that time; although we believe that all such had had it, and practically all claimed to have had the "fever of acclimatization." Much pains was taken, and the data used in this investigation are believed to have been reliable. The sources were: (1) the books registering the arrival of the *Peninsulares*; (2) the records of the yearly registration of their *cédulas*, by which we could tell whether the man registered was in Havana at the dates, a year apart, of these inspections; (3) the books of the Sanitary Office reporting communicable diseases; (4) the registers of the hospitals; and finally (5) the records of the cemeteries. Nearly all those recorded as having yellow fever developed it within the first two or two and a half years after arrival. Those recorded with yellow fever after the third year were few indeed.

Consideration of the findings in this study—indicating that a large majority of yellow fever cases escaped recognition, even in Havana—was instrumental in the change of plan, in June, 1905, of Gorgas' yellow fever campaign in Panama, transferring the emphasis to control of the insect host instead of the human host, which previously had been the main objective.

#### OTHER CARIBBEAN REGIONS AND BRAZIL

Leaving Cuba, the history of which implies the existence of yellow fever in Vera Cruz and other places in the Caribbean area in 1648 and thereafter, we find, in immediately succeeding years, fairly frequent mention of epidemics in this region. Some of these, however, have been accepted as yellow fever on insufficient evidence,<sup>5</sup> and, while it is certain

<sup>5</sup> An instance of this is the account which Béranger-Féraud (*op. cit.*, p. 30) gives of an epidemic on the island of St. Lucia, which, from his report, has become somewhat famous in the annals of yellow fever by reason of its extraordinary mortality. Of this he says, citing du Tertre as his authority: "In 1665 an English squadron which was in good health took possession of *Sainte-Lucie* and placed there a garrison of 1,500 men; by the month of January of the following year this garrison was reduced to 89 as the result of a violent epidemic of yellow fever." He does not locate the passage in du Tertre from which this is taken, but a careful



that the disease continued to prevail in this region, it is not until 1686 that we find another extensive epidemic fully described. In this year appeared in Brazil the great epidemic of "*a bicha*,"<sup>6</sup> the account of which,

---

search of this author's work shows only the following allusion to this disastrous occupation of St. Lucia (du Tertre, 1667-71, vol. iii, pp. 243-244): "While these piracies (*brigandages*) were going on in the islands, to the great injury of the Company and the inhabitants of these islands, six deputies from the Governor and the inhabitants of the island of *Sainte-Lucie* arrived in a boat at Martinique, and declared, on their part . . . that they had taken possession of that island believing that they were doing no wrong: but that God had made known to them that they had usurped the rights of others, by the punishments which he had visited upon them, which was (*sic*) such that dysentery (*le flux de sang*), famine, wars and the continual incursions of the savages had reduced them from 1,500 to 89, and very humbly prayed these gentlemen to take back this island and all that belonged to them, and to give them boats to transport them to another land." He adds that an expedition for this purpose was made ready, but on arrival, January 18, 1666, found the island abandoned, the inhabitants having been taken off by an English pirate. While yellow fever may well have been among the "*chastimens*" suffered by these renegades, there is nothing in du Tertre's account to identify it, still less to warrant charging against it the whole of the mortality.

Béranger-Féraud is among the greatest of the systematic writers of his generation, combining most happily his own experience with extensive and well digested research in the experiences of others. He ranks high even among the great French writers on yellow fever. Yet he is, I think, entirely too prone to accept any epidemic, especially if occurring in the Americas, as yellow fever: that is, his personal equation in this matter is positive.

<sup>6</sup> The writer is informed by Dr. E. J. Scannell, of the International Health Board (personal communication) that this name "*bicho*" or "*bicha*" (worm or beast) is now vulgarly used in certain parts of Brazil for dysentery. Apparently it is a survival from the past, when intestinal worms, found at autopsy, were supposed to be the cause of the disease. In the island of São Thomé we find that in the seventeenth century the same name, "*bichos no cu*" ("*no cu*" meaning "in the breech") was applied to a conglomerate group of diseases in which dysentery was prominent. Also, Sigaud (1844) refers to a disease "*bicho*," prevalent in Brazil, which, from his description, may have been, or included, *schistosomiasis mansoni*.

That a name of this significance should be applied to yellow fever seems remarkable. The explanation suggested by Dr. Scannell, and which seems reasonable, is that, autopsies having been made on some of those dead of yellow fever—because it was a strange disease—intestinal parasites were found—as would almost certainly happen, and that from this came the name "*a bicha*." This suggestion finds some support in the account which Dr. João Ferreira da Rosa, in his *Trattado Unico da Constituiçam Pestilencial de Pernambuco* (1694) gives of an autopsy made in that epidemic—so far as we know, the first autopsy on a yellow fever victim that is recorded. The "anatomy" was performed by Antonio Brebon, a ship's surgeon, whose findings are given as recited under oath in support of his claim to have discovered the cure of "this contagion." He "found the liver corrupted in the

by da Rocha Pitta (1730) is almost as good as that of Cogolludo in Yucatan.

According to da Rocha Pitta, himself a Bahia man, the epidemic developed first in 1686 in Olinda—adjacent to Recife—whence it was introduced the same year into Bahia, where there was a severe and general epidemic—evidently in virgin soil. After some years of prevalence, the disease seems to have died out in Brazil, to be reintroduced in 1849, from either New Orleans or Havana.

It seems possible that this epidemic may have been introduced into Brazil from the Cape Verde islands by the fleet of Gomez Freyre de Andrada. This fleet, leaving Lisbon early in 1685, had stopped at the Cape Verde islands—"a place notoriously unhealthy,"—and en route thence to Brazil had lost many men of a sickness. In Brazil they went first to Maranhão and later—we do not know how much later—to Recife for refitting. It was after this that the epidemic broke out in Recife, but we do not know the interval, nor do we know the nature of the sickness in Andrada's fleet. Hence, that this was the origin of the Brazilian epidemic is only a possibility—not at all established by the evidence we have. Certainly other avenues of infection were not lacking, and indeed, unless the insect vector were absent, or at least insufficient, in earlier years, it is difficult to account for the late appearance of yellow fever in Brazil.

In 1690 began in Martinique an epidemic of which we have a full account by Père Labat (1722). It was brought quite certainly from Recife to Martinique by the *Ori flamme*, a French war vessel which had cleared from Bangkok, but had touched at Pernambuco. Because of this origin, the epidemic, which spread generally over the West Indies and the continental shores of the Caribbean, became known as the "epidemic of the *Ori flamme*," or the "*mal de Siam*." Indeed it gave the latter name, "*mal de Siam*," to the disease for a long time and over a wide area, much of which was infected from other sources and with a fever absolutely unknown to Siam.

---

interior part and that it was of a different color from the natural," and, in the stomach, "a quantity of viscous humor of a black color like soot." Yet in spite of these findings, from which we, of the present day, fairly surmise the cause of death, he is chiefly impressed by the finding of worms ("*lombrigas*") in the intestines, and announces these as the cause of the disease. His "remedy" includes "certain potions from the use of which the sick cast out the worms by mouth and by the lower way." It is only fair to add that Ferreira da Rosa accepts neither the cause nor the cure.



From this date on the disease is generally recognized—at least when occurring in epidemics—and there is such abundant record of epidemics, too numerous to mention, that it seems unnecessary to follow them up. It was, however, some years before the name “yellow fever” came into general use as distinguishing this disease from malaria. The earliest use of “yellow fever” in this sense seems to have been in 1750 in Barbados by Griffith Hughes, though long after that we still find other writers using “yellow fever” in the older sense. In these years of confused nomenclature we find this disease described under many names. Most often these refer to the place—or supposed place—or origin, as “fever of Olinda,” “epidemic of the *Oriflamme*,” “*mal de Siam*,” “fever of Bulam;” but sometimes they are of other derivation, as “*a bicha*” in Brazil. There was also used at one time on the south shore of the Caribbean (Ulloa, 1748) a name descriptive, indeed, but of the epidemiology of the disease, not of any appearance or symptom. This was “*chapetonada*,” because the fever was apparently confined to “*chape-tones*”—newly arrived Europeans.<sup>7</sup>

The name finally adopted for this disease by the Spaniards, “*vómito prieto*” or “*vómito negro*,” more distinctive than the English name “yellow fever,” is quite certainly older. The fever that came to Havana in 1671 was stated to be so known in Vera Cruz, whence it came. There is a statement by Ulloa (1748) implying that this name was applied in 1729 to this fever at Cartagena de las Indias and at Santa Marta, and presumably at Porto Bello, where it had long been known even if not named. This was the fever that “caused many losses in the fleet of Pintado” and was carried by it to Cadiz, producing the epidemic of 1730, believed to have been the first entry of yellow fever into Europe.

<sup>7</sup> As if to show that there is no new thing under the sun, or rather the universal likeness in the minds of men, this was repeated in the name “society fever” of Jacksonville, Florida, in the spring of 1888, because the fever there attacked especially the northern tourists new-come to the town.

## CHAPTER XIII

### AFRICA: GENERAL CONSIDERATIONS

We think that we have shown from historical and epidemiological data that in only two regions, tropical America and West Africa, were the climatic and sociological conditions such that yellow fever could, in early times, have been permanently endemic, and that, with the earth as we have known it in historic times, in one of these two this disease, as we now know it, must have developed.

We have presented such data, negative, it is true, but we think convincing, to show that yellow fever was not present prior to the coming of the Spaniards in those parts of the Americas in which the conditions were suited for its permanent endemicity—the Maya countries, and the lowlands of the Aztec Empire. Nor have any data been found to induce us to believe that it existed at this time in other parts of the Americas—the West India Islands or the Caribbean coast of Central America east of the Mayas or elsewhere—none of which, indeed, offered such suitable conditions for the continuance of this infection as the two regions first named.

If, then, the data presented are such as to limit the origin of this disease to the two great regions mentioned and to exonerate the New World as the place of origin of yellow fever, West Africa becomes the *necessary* alternative. This deduction is based on historical and epidemiological evidence alone. If, in addition, we consider the argument for Africa's being the place of development of *Aedes* (*Stegomyia*) *aegypti*, and the comparative reactions to yellow fever of negroes and amerinds, this conclusion is strengthened—we consider much strengthened.

Nevertheless, it is purposed to examine such historical evidence as we have on West Africa, to see what light it throws, positive or negative, on this question. It seems advisable to limit this inquiry as to the early presence of yellow fever in Africa both in time and place.

In time we need not go beyond 1648, because we know that in that year yellow fever occurred in America, in Yucatan and the French Antilles. Hence, its presence in Africa later than this might have been due to an importation from America.



## AREA TO BE EXAMINED

For a possible place of origin of yellow fever in Africa we have limited our quest to West Africa. This because, within historical times, the disease has never been known to be established in any other part of this continent, although one thinks that, were the causative organism introduced, the eastern coast of Africa now presents, and may have long presented, all the other conditions necessary for the permanent endemicity of this disease—possibly as much so as the west. Nevertheless, it has not been noted on the east coast.

*Senegal and Gambia*

On the west coast, one feels like limiting the area to be examined approximately to the Gulf of Guinea, say from Sierra Leone to the mouth of the Congo. It might be extended north to the Great Desert, as yellow fever has frequently been noted in Gambia and Senegal. The epidemics here, however, have, in general, lasted only one or two seasons each. There seems, then, the same reason to exclude this part of the African coast from our inquiry as there is for excluding Louisiana and South Carolina from that in America. In both regions there may have been many epidemics, but in neither has the disease shown a tendency to become permanently endemic. Since in these countries, Senegal and Gambia, *aegypti* have probably existed for a very long time, epidemics may have occurred there from time to time in prehistoric or early historic days as they have done since yellow fever was there recognized. Whether the frequent disappearances of the infection on this part of the African coast were due to climatic conditions or to failure of the human host, or possibly to a combination of the two, may be a question. To whatever cause or causes due, one knows of no reason to believe that conditions were more favorable to the permanence of yellow fever here in prehistoric than in modern times. Probably they were less so, as communications along the coast and with the hinterland by river have been much better since European factories were established there than in times past, and in late years railways have been built, which are believed to have played a part in the spread of recent epidemics of yellow fever (Gouzien, 1922). The hinterland of which we have spoken, is, and for a very long time has been, quite thickly populated; and obviously, the greater the number of communities brought into communication with each other, and the larger the size of such communities, the greater is the chance of the permanence of this infection.



FIG. 5. WEST AFRICA AND AMERICA: REGIONS OF





With the establishment of the European (French and English) "factories" on this coast, the trade routes, east and west, between the hinterland and this coast greatly developed. Prior to this the greater part of the commerce passed north and south; north with the Desert tribes of the Sahara, and through the Sahara with the Barbary States: south with the Soudan, and beyond it, possibly even to the hinterland of the Gulf of Guinea.

Jenné and Timbuktu, both in the northern Soudan, were the commercial centers for the concentration and distribution of this trade. That distributed from Timbuktu was practically all going north and south. To the north it was carried in caravans by the Desert tribes, the "Azanegues" of Azurara—the modern Tuaregs. To the south, down the valley of the Niger, from which river Timbuktu is about twelve miles distant, it was carried by a people of the Soudan, the Mossi, "greatly employed in trading."<sup>1</sup>

Jenné, from which the name "Guinea" is derived, is older, and, in earlier times, say up to 1200 or 1300 A. D. at least, was more important than Timbuktu. It is nearer the coast; and although its trade was north and south, it was east and west as well—hence to and from the coast. This east and west distribution of its trade made Jenné the metropolis for this part of the west coast, and during the fourteenth and fifteenth centuries gave its name, Guinea, to all of it from Cape Bojador (Bogador) south. Unless one keeps this in mind he will be very much confused by some of the early chroniclers.

Salt going south, slaves, ostrich feathers, and gold coming north in exchange, were the staples of this traffic, that of salt being especially important.<sup>2</sup>

### *The Gulf of Guinea*

On the coasts of the Gulf of Guinea, taking this section as a whole, say from Sierra Leone to the mouth of the Congo, yellow fever appears to

<sup>1</sup> Marmol (1667, vol. 3, p. 75), in the seventeenth century, places this people further to the south and west: about 140 leagues to the interior from Cape Palmas in Upper Guinea, below Cape Verde.

<sup>2</sup> The theory of the need of negroes for salt was that the intense heat of the Soudan—which had already blackened their skins and crinkled their hair—would, without this preservative, lead to "corruption of their blood and decay of their bodies," and their thick lips and fleshy noses—compared with the thin-lipped, aquiline features of the desert dwellers—were instanced as this process hardly restrained even by the salt consumed. This is given, and with credence, by the early Portuguese from the reports of the Azanegue salt merchants (Ramusio, 1563-83, vol. i).



have been continuously, or well nigh continuously, endemic since it was first reported as being there in 1793 ("fever of Bulam"). This being the case, it seems obvious that the sociological, climatic, and biological conditions there were such, in modern times, as were suited to insure its permanence. And they seem to have undergone little change within historic times which would affect the existence of yellow fever. We believe *Aedes aegypti* to have been long present, and since we have known this region, it has been thickly settled and with large, or very large, towns. Their inhabitants were great traders, hence with much intercommunication among themselves. Save for increased communication by sea—with the outside world and with each other—there has been, one thinks, little change in historic times affecting the existence of yellow fever among them. Hence, if present at any previous time, it should have remained permanently endemic.

#### RECORDS

The history of this part of West Africa presents rather a contrast than an analogy with that of those parts of America which we have discussed. The native records of Mexico, the Maya country, and Peru, although scanty, are reliable and fairly complete for a period long before the advent of Europeans. There are absolutely no native records for this part of Africa. For its history, then, we must rely entirely upon European accounts, and even these show contrast rather than likeness to those for America.

The history of the voyages of discovery of Africa are extremely good, especially those of the Italian navigators in the service of Prince Henry, as given by Ramusio. The Chronicle of Eannes de Azurara, who was deputed by Prince Henry to write an account of them, also is full, definite, and unquestionably correct, at least as things were reported to him. It is a contemporaneous account, completed, or rather stopped, in 1453, and covers the time up to and including 1448, the "Voyage of Vallarte, the Dane," of that year, being the last recorded. Barros, too, gives a very full account of the history of the Portuguese in West Africa, not so full as Azurara's, indeed, for the time the latter covers, but continuing the history well beyond the early settlements. His account, we think, is derived mainly from official documents—that is, at third hand. We think his history is in the same class as that of Herrera for Mexico. These accounts compare fairly well with the accounts of the Spanish voyages of discovery in America, but are far less informative sociologically.

The history of the settlements themselves is far otherwise. In America, Europeans settled in large numbers and in many places, Hispaniola, Panama, Mexico, Yucatan, Guatemala, Bogotá, Peru, etc., and established Spanish colonies—that is, permanent Spanish communities. Much of the plateau country in America was healthy. Here the Spaniards mainly settled, and it early supported a considerable and permanent Spanish population mixed with the indigenes. Indeed, no great complaint of unhealthfulness is made of much of the lowlands until malaria appeared, and on them, too, there were early considerable flourishing and permanent Spanish settlements. These were true colonies, immigrants coming to stay permanently and their descendants to stay after them.

There was thus intense European interest in the American settlements, and much was written about them. Peter Martyr d'Anghierra translated for the Pope practically all the personal letters of Columbus to himself, giving Columbus' account of the New World. Oviedo made the determination to write his great history in 1493—within one year of the discovery. Herrera was appointed royal historiographer for the New World and has given us a very full account based on official documents, supplemented, it may be, to an extent, by his imagination. Las Casas, early present on the ground, began his history as a labor of love for the Indians. Probably thirty titles would scarcely include the early accounts of the Spanish settlements in America, by eye-witnesses, or by writers who received accounts from eye-witnesses.

The early European settlements on the West African mainland, however, were all small, being mere trading posts, rather than true colonies. Angola and, to a less extent, São Jorge da Mina came nearest to being colonies. Moreover, even the trading stations were rarely any great distance inland, not even up the rivers.

The settlements on the African islands were, indeed, different. These were true colonies; and we are more apt to have note of a disease in a true colony than in a trading post, especially if the disease be one foreign to the mother country of the colonists. Yet even the island colonies were not large and, except in the first settlement, Europeans must have arrived in small numbers at a time, which would render little noticeable among them any endemic disease producing immunity by a single attack. An *epidemic* of such a disease, endemically established, would be *impossible*. Their histories, too, are scant and mainly commercial, with nothing like the wealth of sociological information we have on early Spanish America.



Obviously, the negative value of the histories for these two regions, Africa and America, is very different. For the native records, when neither the Nahuatl nor the Mayan records made mention of anything that could be regarded as yellow fever, the writer made the deduction that yellow fever had not existed, because the records are of such character that he felt convinced that an epidemic of yellow fever, had it occurred, would have been recorded. The fact of there being no native records of an epidemic in Africa would in no wise warrant such a deduction, as there is no native record of anything.

The European history of this part of Africa, too, is scant and, as we have stated, mainly of discovery and commerce; hence, its failing to mention or give details of sickness would be far less significant than a similar omission from the more complete American histories.

#### DIFFICULTIES OF RECOGNITION OF YELLOW FEVER IN AFRICA

Moreover, had the histories been equally good, yellow fever occurring in Africa had been less apt to be mentioned than in America. It would not have been noticed among the negroes, especially if in a permanent endemic focus. Among Indians, as happened in Yucatan, it was as severe as among whites, and *compelled* notice. At no place on the African mainland was there, in early times, any considerable number of Europeans, and these, except at first settlement, generally came there a few at a time. In early times, too, many of the colonists accounted white were of mixed Portuguese and negro descent, among whom the recognition of yellow fever had been, it may be, well nigh as difficult as among pure negroes.

At the island of São Thomé the Portuguese population was greater, but, except at the beginning of the settlement, none of this immigration came in large numbers at one time and, as we have said, it is only among Europeans, and among new-come Europeans, if the infection were fairly continuous in a place, that yellow fever would be noted. Thus, because of the mild reaction of the negro to yellow fever, and the small numbers of susceptible whites exposed at one time, the manifestations of yellow fever in Africa would be decidedly less definitive than in the New World. Certainly, one would not here expect the clear-cut histories that we have of the fever of 1648 in Yucatan or that of 1686 in Bahia, which, indeed, are not only recognizable, but unmistakable.

Moreover, malaria was very prevalent and very severe in West Africa, and it is improbable that we could, from the descriptions of early times, differentiate yellow fever from the omnipresent and ever-present malaria,

even among whites. Indeed, this was not done until 1778, by Schotte, at St. Louis de Senegal, when he had an opportunity to observe a severe *epidemic* of yellow fever in a considerable body of British troops.<sup>3</sup>

On all three of these counts, then, (1) the small number of Europeans, among whom only could yellow fever be recognized, (2) the prevalence of severe malaria, making it difficult of recognition even among them, and (3) the paucity of historical record, it seems obvious that not recording yellow fever in Africa would be of far less negative significance than the same would be for America.

#### CRITERIA FOR IDENTIFICATION OF YELLOW FEVER

It is not easy to determine what criteria we must look to here as indicating the presence of yellow fever. The mere occurrence of sickness among newly-arrived Europeans, and even of severe sickness, is not sufficient, especially here, on account of the prevalence of malaria. If such a sickness showed a very high case mortality or a very high community mortality in a short time, this might be taken as indicative of the more deadly infection, especially if it died out in a short time—determinable by the size of the community and the introduction of newcomers. A positive determination in such a case, however, would require the evidence to be full, definite, and reliable—a thing we are little likely to get here at these times—and a very careful judgment in the weighing of it.

Possibly the best criterion is the occurrence of epidemics of high fatality aboard ships on the high seas after having been in African ports, especially so if the sickness continued to spread among the crews for any length of time. Also, should we find evidence of the immunization of Europeans to a severe prevalent sickness by a previous attack of an apparently similar illness or from having lived in a place where such a sickness prevailed, this evidence would be of value in proportion to its definiteness and reliability. It would, however, be complicated by the presence of malaria, repeated attacks of which occasionally give a fair degree of immunity to itself; and the occurrence of a severe attack of

<sup>3</sup> In fact, it can hardly be said that Schotte differentiated the two diseases, for though he gave to the yellow fever that he saw a distinctive name (*synochus atrabiliosa*), and left a description which is to us definitive, he considered it an intensified form of the malarial fevers. Lind reports outbreaks of what may have been yellow fever in Senegal and Gambia in earlier years, but there is still some question which of these—if any—were yellow fever. Lind himself, an erudite man, and with much experience in tropical diseases, considered all the above malarial fevers.



malaria, whether in one immune to yellow fever or not, might well be taken for yellow fever. The data to be derived from this, then, would be far less satisfactory than in a place where malaria was less prevalent and less severe.

Just as in modern times there has never been an epidemic of yellow fever in this part of Africa comparable to the epidemics which have been of frequent occurrence in the Caribbean region, so we would not expect, in early times, to have had one here like that of Yucatan in 1648 or of Bahia in 1686; and, except by the distinctive features which it shows in frank epidemics, we can hardly expect to be able to identify yellow fever with certainty, from scant historical records, even where it may be present, and indeed, highly prevalent. We begin our examination of Africa, then, recognizing that positive evidence of early yellow fever will be less likely discoverable, and negative evidence of less weight than in the New World.

## CHAPTER XIV

### WEST AFRICA: DISCOVERY AND SETTLEMENT

It has been plead that if yellow fever had been so long present in Africa, Europeans should have had cognizance of it long before they did, because their acquaintance with Africa dates from the dawn of European history. One might almost as well say that if it had been long present in America, it should have been known to Europeans from the time of Lief Ericson. That we have history of Africa from extremely early times is indeed true; but up to the latter half of the fifteenth century we have none for the part of Africa that we suspect of being the place of origin of this infection. Egyptian history, of course, goes back to the very early times, and with Egypt, I presume, we should include, to an extent and indirectly, Nubia and Ethiopia, to the south. Africa north of the Great Desert, the so-called "Barbary States," was also well known; part of it to the Greeks, all to the Romans. Arab traders from the Red Sea and the Persian Gulf in the tenth century went down the east coast as far as Mozambique and Madagascar, founding settlements, and bringing much of the country into limited and indirect communication with the Levant. There is also record of a much earlier expedition down this coast, that of Eudoxus of Cyzicus, about 130 B. C. However, all this relates to parts of Africa where, in historic times, yellow fever has never become established.

### ANCIENT KNOWLEDGE OF THE WEST COAST

For the west coast of Africa, it is a question what credence should be given to the reported expedition of the Phoenicians, made for Pharaoh Necho, 600 B. C., from the Red Sea down the east coast, returning through the Mediterranean. This voyage is reported to have lasted over two years, the expedition stopping ashore and raising crops of grain for revictualling. The Greek historians and geographers are quoted as rejecting it, and on the very grounds which would make us inclined to accept it—that making the turn of Africa, they had had the sun on their right. Herodotus, knowing of the sun always to the south of the place of observation, naturally believed it would be on the left of the observer going to the west. South of the equator, rounding the



Cape of Good Hope, of course, the contrary would be the case (Saco, 1879).

In Walckenaer's *Collection des Relations de Voyages . . . en . . . l'Afrique* (1842, vol. ix, pp. 379–380) is an interesting statement as to the presence, in upper Guinea, of certain pottery beads, the so-called "aigris" beads, which are accounted of Phoenecian origin. A. B. Ellis, too, in his *History of the Gold Coast of West Africa* (1893), states that near Wassaw, in the Gold Coast, where neither Dutch nor Portuguese had ever been, old workings are found in two tunnels following gold veins. He makes mention also of "aggry" beads which, he is told, are characteristic of Sidon, as being occasionally found in gold-producing districts, and only in such districts. Other modern writers mention these beads, and there seems to be rather general agreement that they are occasionally found in the Gold Coast and its hinterland. Naturally, the above are meant to imply more than a mere expedition, rather a commercial intercourse, probably especially for gold.

Better authenticated is the voyage of Hanno, the Carthaginian, about 570 B. C., going south from the Pillars of Hercules down the west coast to "the Southern Horn," and returning by the same route. The account we have of this voyage is "The Periplus of Hanno," a Greek translation, made about 300 B. C., from a Punic inscription upon a bronze tablet found in the Temple of Moloch at Carthage. It naturally, then, is not diffuse in its detail (see Bunbury, 1883, vol. i, p. 322; and Saco, 1879, appendix 2). Modern writers have tentatively fixed the "Southern Horn," the southern limit of Hanno's voyage, as a headland of Sherboro Island, Sierra Leone. Here he turned back, he states, from failure of provisions, but one judges also in awe of the supposedly supernatural manifestations which showed about the end of his journey. There is indisputable internal evidence, in the extremely brief account that we have of this, that Hanno sailed along the forest region of the West African coast well south of the Great Desert, and possibly visited Madeira. This expedition, however, can not be accounted as opening communication between West Africa and Europe.

Great sailors as they were, the Berbers of North Africa made little or no exploration of the West African coast, confining their voyages to the more profitable raids in and across the Mediterranean, and, indeed, they regarded Cape Cantin as the end of the possible, or at least of profitable, navigation to the southward. There was, of course, caravan trade across the Sahara with the northern Soudan, and thus indirectly with the country south of it, but this was in relays, and hence was so

indirect that it could scarcely be called communication of this section with European, or even North African, civilization.

#### VOYAGES OF THE "DIEPPOIS"

The writer has examined with some pains and at length—wasting time, he thinks, for there is considerable literature on them—the claims for very early discoveries and settlements in West Africa by the Normans ("Dieppois"), such as their alleged establishment of *Petit Dieppe* on the Grain Coast in 1364, of *Petit Paris* a little later on the same coast, and of *La Mime* (Elmina) in 1381.

Much curious, but the writer believes generally erroneous, information—in statement of fact or in deduction therefrom—is given on this subject by Villault de Bellefond (1669), Labat (1728), Estancelin (18—), Vitet (1844), Gaffarel (1880), and Asseline (1874). To the writer, however, the evidence seems absolutely unconvincing. The claims made by Villault de Bellefond were not put forward until after his voyage to Africa in 1666 and 1667. Also, a number of the allegations made by him, as well as by Labat, Gaffarel, and others, are quite certainly erroneous; and those that are true are susceptible of another explanation, namely, that the French had, indeed, traded on this coast before 1666, but at much later dates than those alleged.

This whole question has been examined by Walckenaer (1842, vol. ii), who concludes, as does Beazley, high authority on early African history, that the evidence is not valid. Obviously, then, the claim that communication between the coast of Guinea and Europe was set up by the Normans in the fourteenth century is not proven; and it is, we believe, fallacious. The same may be said, one thinks, of the allegation of the discovery of America by the men of Dieppe not long before Columbus.<sup>1</sup> Certainly the Dieppois were bold and enterprising seamen,

<sup>1</sup> Among the claims of Dieppe for priority in discovery, it is alleged that a vessel sailing from Dieppe, in 1488, for West Africa and general discovery, reached the coast of Brazil at the mouth of the Amazon, returning to her home port the following year, and that among her crew was one Pinçon, who was dismissed and his license as navigator taken away for insubordination. He, it is claimed, was the Vincente Yañes Pinzon who, with his two brothers, accompanied Columbus as pilots and who advised that his course be directed to the south instead of due west from Palos—indeed more to the south than the Admiral would agree to—looking for the Brazil that had already been visited.

Of the claims made for the Dieppois, this seems intrinsically the least improbable, for the route from West Africa to Brazil was short, and with a fair wind all the way.



as their early fisheries off Newfoundland attest, but that they made *all* the maritime discoveries of the fourteenth and fifteenth centuries, forestalling those of Prince Henry's men, Columbus, Diaz, and da Gama, one is scarcely prepared to accept.

#### EARLY PORTUGUESE DISCOVERIES

Leaving aside all questionable claims, we have dependable historical evidence that about the middle of the fifteenth century communication between Europe and the west coast of Africa began to be opened up by the Portuguese under the patronage of Prince Henry the Navigator. In 1434 Cape Bojador was rounded by Gil Eannes; Cape Nun, well to the north, had been visited previously by the Catalans; in 1441 Cape Blanco was rounded; and in 1444-45 Dinis Diaz reached the Senegal river. In 1445 Cape Verde was passed, again by Diaz; and the river Gambia was reached the next year. Then, in 1455, began the voyages of Aluise Ca da Mosto, a Venetian nobleman in the service of Prince Henry, who discovered the Cape Verde Islands and ascended both the Senegal and the Gambia for considerable distances. Of these we have a beautiful account in Ramusio's great work, *Navigazioni et Viaggi* (Venice, 1563-83, vol. i). Finally, in 1488, Bartolomé Diaz passed the southern end of the continent and, turning north, reached the mainland—to the east of his course, at a point north of the present Port Elizabeth. He had passed the southern point of the continent too far off to see it, but sighted it on his return and called it the "Cape of Storms." The King, however, renamed it the "Cape of Good Hope." There is much in the point of view.

This was followed, in 1497-1499, by the voyage of Vasco da Gama to India by the same route, a crown fitting and sufficient to the great work begun by the Portuguese Prince a little over sixty years before, a beautiful work, well conceived and brilliantly successful.

#### DISCOVERY OF THE ISLANDS OFF THE WEST COAST

While communications were thus being established between Europe and the mainland of West Africa, the islands lying off that shore were discovered—or it may be, in some instances, rediscovered. The Canaries, the only group that were inhabited when discovered, were occupied by the Spaniards, beginning in 1402. The others were discovered by the Portuguese—the Madeiras in 1418; the Cape Verdes in 1456, by Ca da Mosto; and São Thomé in 1470. They were all settled in the fifteenth century, and European communication with them was, in gen-

eral, more active than with the mainland. Also, by reason of their location and their relation to the slave trade, they came into early communication with America.

#### BEGINNING OF THE SLAVE TRADE

In his patronage of voyages of discovery Prince Henry was actuated mainly by a desire for geographical knowledge, and spent the bulk of his own fortune in these expeditions, but their cost was offset to an extent by the slaves brought back. The first captives from these expeditions, brought to Portugal by Antam Gonçalves, came in 1441. These were "Moors" from the Sahara, Berbers by race, later called "Azanegues," the modern Tuaregs. It was soon found that one of these could be exchanged with his friends for several negroes ("two Moors for ten Negroes" is mentioned), each intrinsically more valuable to the Portuguese than the Moor, and the Azanegues were then, when captured, returned and exchanged. Negroes, too, were purchased from the Azanegues. Later, when the Portuguese had reached the country south of the Senegal, the slaves were all negroes, the great majority of them acquired by purchase from negro owners. This was really the beginning of the African slave trade as it was known in comparatively modern times.

About 1451 Prince Henry strictly forbade slave raids or any violence to the natives. He returned King Bezeghichi without ransom; and sent back other captives who had been brought to Lisbon, "that thereby they might be less bitter against the Christians, . . . trade with us and inform us of their countries." These orders were carried out by men like Ca da Mosto and Diaz, who were as much interested in geographic science as Prince Henry. Indeed, from the accounts of these voyages that have come down to us, it was only in the earlier ones that freemen were captured for slaves by the Portuguese. Later, the slaves seem to have been purchased. According to Saco (1879, p. 30), quoting Barros (1628), the first negroes (freemen) taken by the Portuguese in their own land were four captured in a canoe in the Senegal by Nuño Tristam in 1446. All previously had been bought from the Azanegues. As the slave raids were ordered stopped in 1451, no great number of negroes could have been thus taken. A large number, however, were bought—and for a long time—from their negro masters.

#### EARLY SETTLEMENTS ON THE MAINLAND

*Arguim.* In 1448 a small fort, as a base for the slave trade, was built in the Bay of Arguim, just south of Cape Blanco. This was the first settlement on the west coast of Continental Africa made by Europeans.



*Elmina.* The next settlement after Arguim was at São Jorge da Mina (the present Elmina) where, in 1482, Diogo d'Azambuja built a fort and established a permanent station. Although more of a colony than anything hitherto attempted, this was a permanent trading post on a large scale rather than a colony in the true sense, no attempt being made to build up a Portuguese community with permanent immigrants and their descendants, as was done by Europeans in the Americas. Still, this was the first important, permanent establishment of Europeans on the west coast, Arguim, a small fort, being intended only as a base of operations for the local slave trade.

*Angola.* In 1490 the Portuguese established themselves in Angola, in lower Guinea, where they built a fort and a church. This was different from their other stations in Africa. The king of that region accepted Christianity and received the Portuguese as a kind of superior friends; and they seem to have exercised a species of suzerainty over the Africans in this country and far to the southward. The African ruler sent an embassy to Lisbon, which compliment was returned; the sovereignty of the African king was recognized; protection was later extended when he was attacked and defeated by a tribe of cannibals, Portugal assuming in everything that his relation to the King was that of a feudatory to a suzerain.

Whether this properly could be called a colony by our definition may be a question. There was never any attempt to make a considerable Portuguese community from permanent immigrants and their descendants. The garrison, however, was, for this part of Africa, large, being mentioned at one time, although much later, as comprising 450 men; and it became the center of administration of a large territory in Lower Guinea, over which Portugal claimed suzerainty and the exclusive right of trade as against other Europeans. Angola, however, was primarily a trading post and the center of the slave trade for this region; but it was for a long time much the largest post on the Guinea Coast, upper or lower.

Following the Portuguese on the mainland, other European nations established themselves on the African Coast; first Holland and France, then England, Sweden, and even Brandenburg. These establishments, too, were trading posts, not colonies.

#### ISLAND SETTLEMENTS

The conquest and settlement of the Canary Islands, though not completed until the island of Teneriffe was brought under subjection, in

1495, was begun by the Spaniards in 1402, hence this may be counted the earliest of the island colonies. The Madeiras were settled in 1420, and the Cape Verdes in 1460–61—both under license from the Portuguese Crown. Settlement of São Thomé was begun in 1485, but the attempt failed until Jewish *déportés* were sent there, in 1493.

Unlike the mainland settlements, which remained small, those on the islands rapidly developed into colonies of considerable size. They became, too, ports of importance; and, especially by reason of the slave trade, the Cape Verdes, the Canaries, and São Thomé were brought into close and constant communication both with the west coast of Africa and with tropical America. We shall, therefore, have occasion later to discuss in more detail the history, both during settlement and later, of these colonies.



## CHAPTER XV

### EARLY RECORDS OF SICKNESS ON THE WEST AFRICAN COAST

#### SICKNESS DURING DISCOVERY

The history of the Portuguese voyages of discovery is unusually good in everything relating to the geography of the West African coast and the ventures there, Azurara's Chronicle (1489) and those in Ramusio's Collection (1550-59) being models in this respect. In them, however, up to 1482, the occurrence of sickness is mentioned only once, and then incidentally. This is in Ramusio's account (*op. cit.*, vol. i, leaf 108) of the second voyage of Ca da Mosto, in 1482.<sup>1</sup> Having ascended the river Gambia "for about 60 miles" to "the country of Battimansa," Ca da Mosto halted, sent a deputation to the chief, and received on board large numbers of natives. He then says "...at the end of the eleventh day we decided to leave and to go to the mouth of the river, because many of us had begun to suffer with hot (*calda*) fever which is acute and continuous."

No further details are given of the sickness, and the context implies that, unless it had affected further progress of the venture, the sickness would not have been noted at all. If this were true of Ca da Mosto—easily the most cultivated of the servitors of Prince Henry—one would expect it to be accentuated in the others.

Sickness, then, would, or might, scarcely be noted in these voyages unless it seriously affected the progress of an expedition. The crews of the vessels engaged in discovery and making slave raids were larger than those on merchant vessels, which only carried enough to work the ship under ordinary circumstances; and thus a larger number of the first could be incapacitated without seriously inconveniencing the voyage. Still, had there been even approximately the amount of sickness which showed later in the expeditions of Windham, Bird and Newton, and others, it had seriously interfered with these expeditions and would have been noted. I think we must conclude, then, that there was little sickness of a serious nature in these early expeditions.

Unquestionably a factor—we believe the determining factor—for the difference between these and later expeditions, is that the former were

<sup>1</sup> See also Walckenaer, 1842, *op. cit.*, vol. ix, pp. 353-367.

mainly confined to the seacoast, anchoring off shore and very rarely ascending rivers or going inland, while the contrary was the case with later expeditions, in which so much sickness occurred. Note that in the case quoted for Ca da Mosto the sickness occurred well up a river.

#### SICKNESS IN COMMERCIAL VOYAGES

As illustrating what we have just said of the more prominent mention of sickness in commercial voyages than in those of discovery, the only account we have been able to find of an early commercial voyage from Portugal reports much sickness, and that in a region in which none was noted in a military expedition made a little later.

After trade with the Guinea Coast was allowed by concession, a contract was given, in 1469, to Francisco Gomez, a merchant of Lisbon, to trade for five years with the coast south of the Cape Verde Islands, a condition being that he was to discover and explore fifty leagues of coast beyond Sierra Leone and, in this exploration, to look for a mine whence the negroes of this coast drew their gold. He fitted out an expedition for trading and incidental discovery under Juan Santarem and Pedro Escobar, both celebrated navigators of their day; and in 1471 he determined on a place which he called "*A Mina*" (Elmina) as the region in which gold was at least procurable.

Of this expedition, Saco (*op. cit.*, p. 41) quotes Andrés Bernáldez as follows: "In the said year of 1471 the flotilla of the said King Don Alonzo discovered the mine of gold which today the Kings of Portugal possess, which is on the coast of the Ocean Sea towards the part of the South (*mediodia*) past the coast of the black Xelofes" (*Jaloffes*) "and its boundaries . . . . The major part of the crews became sick and died without remedy and afterwards, pursuing their voyages, the travel became easier and they became well and ceased to die. From which mine of gold very great richness and honor has proceeded to the Kings of Portugal and each day proceeds." Barros (1628) also gives an account of this expedition of Santarem and Escobar, but says nothing of sickness. Possibly more sickness occurred than the Portuguese noted. Bernaldez was a Spaniard.

There are accounts of a number of English commercial voyages and expeditions to West Africa, and to the Gulf of Benin in particular, before 1648—the limit of our inquiry. These were for trading purposes, sometimes to parts of the coast not claimed by Portugal. Abstracts of a large number of these voyages are given by Walckenaer (1842) in his monumental work, but the writer has been at pains to look up the origi-



nal accounts from which Walckenaer's abstracts were taken, in Hakluyt (1885-90), Richard Eden (1577), "Purchas his Pilgrims" (1625), Awnsham and John Churchill (1732), Pinkerton (1808-14), the Abbé Prévost (1747-80), James Stanier Clarke (1803), and others.<sup>2</sup> Hakluyt, Eden, and Purchas made a custom of interviewing returning sea-captains, examining their logs, and getting accounts of their ventures by word of mouth. Their accounts are exact as to dates and locations—matters of special interest to us.

In practically all of these expeditions sickness is noted, and in some, indeed, the mortalities are excessive. For instance, in the expedition of Windham and Pinteado to Benin, in 1553, "scarcely forty" out of 140 men returned to Plymouth; and nearly all the deaths occurred within little more than a month, aboard ship lying at the mouth of the Benin River, dying "sometimes three and sometimes 4 or 5 in a day" (Hakluyt, *op. cit.*, vol. xi, pp. 80-81). The loss in Bird and Newton's expedition, also to Benin, in 1588, was proportionally as great, and in as short a time. It is worth noting that the ships of Windham and Pinteado lay in the Benin River, near the mouth, while a party went up the river "50 or 60 leagues" to "the king of Benin his court" to get their cargo (cardamons). All the sickness recorded was in the men who stayed with the ships. With Bird and Newton, the same procedure was followed for trading, but the only sickness recorded was in the land party.

Such community mortality as these expeditions suffered, and in such a short time—hence not caused by recurrent attacks—is indeed extremely high for malaria, even in West Africa, and it may well be that it, or much of it, was from yellow fever. This, however, is the only evidence therefor, and one hesitates to so pronounce it.

In the third voyage of Towerson to Guinea,<sup>3</sup> a suspicious circumstance

<sup>2</sup> For the voyages of Windham and Pinteado, Towerson, John Lok, and Bird and Newton, the accounts given in volume xi of the Goldsmid edition (Edinburgh, 1885-90) of Hakluyt's *Navigations, Voyages, Traffiques and Discoveries*, are reproduced from the originals in older works; and as this is the most accessible reference, all the citations given are from this source.

<sup>3</sup> The edition of Hakluyt here cited gives the date of this expedition as 1577. The first edition of this work (London, 1589) gives 1557. Other editions available in the Library of Congress give 1557 in their tables of contents and 1577 in the text. Walckenaer (1842, *op. cit.*, vol. ii) gives the date as 1558. The first and second voyages were made in 1555-56 and 1556-57, respectively. In the first, only one death is mentioned (Hakluyt, *op. cit.*, vol. xi, p. 130); in the second, three deaths, on the homeward voyage, are mentioned, and some other sickness is implied (*ibid.*, pp. 145-146).

is that a considerable outbreak of sickness and a number of deaths occurred on one of his vessels on which he had received some sick Frenchmen from a vessel that had been long on the coast. However, while these are the first deaths recorded, there had been some sickness on Towerson's ship a month earlier, and the country they were in was unquestionably malarious, and at places intensely so.

John Lok, who led an expedition to Guinea in 1554-55 (Hakluyt, *op. cit.*, vol. xi, pp. 84-102), mentions the death of "about twentie and foure" of his men, "whereof many died at their returne into the clime of the colde regions, as betweene the Islands of Azores and England." Bird and Newton, too, (*ibid.*, p. 320) leaving Benin April 13 with sick on board, came to the Azores July 25, "where our men beganne a fresh to grow ill, and divers died . . . and as many as remained liuing were in a hard case." This occurrence of sickness on the high seas might have been in men infected after sailing—hence yellow fever. It might, however, as well have been a recrudescence of malaria from striking colder weather. The latter especially seems to have been the case with Bird and Newton, as one gets the impression of few fatalities, but much debility in the crew from this time on, for it is said that unless they had gotten "sixe fresh men" from an English ship "on this side the North Cape," they would have had difficulty in reaching England.

From reasons given before, namely, the malignity of the malaria in West Africa, the small number of Europeans exposed to it in these vessels, and the imperfection of their records (the dates can be relied on—they are from the ships' logs), the difficulty of determining the presence of yellow fever, if it existed in company with malaria, would be extreme. However suggestive they may be, we do not think the data are sufficient, in any of these accounts, clearly, or even very strongly, to indicate yellow fever.

Bérenger-Féraud, indeed (1890, p. 27), accepts the sickness and mortality recorded in several of these expeditions as probably yellow fever, but there is no evidence for this save an excessive mortality among Europeans in a region physically and sociologically well suited for the endemicity of yellow fever and in which we are reasonably sure *Aëdes aegypti* abounded. It would be much easier for the writer to follow the lead of this erudite Frenchman here, but he has set himself to examine the original sources and to use the same bases of criticism for Africa as he did for America.<sup>4</sup>

<sup>4</sup> Bérenger-Féraud, while accepting these as possible instances of yellow fever, does not consider that the disease was indigenous in Africa. He thinks it had been, by this time, imported from America.



## SICKNESS IN EARLY SETTLEMENTS

*Arguim.* There is no account of any sickness at Arguim during the establishment of this post or subsequently. Indeed, except the date, and the purpose for which the fort here was built, there is practically no history for Arguim at all.

*São Jorge da Mina* (Elmina). This was settled by Diogo d'Azambuja, sailing from Lisbon the "Eve of St. Lucia's Day," December 12, 1481, with 500 soldiers, 100 carpenters and masons, and materials for building. The fort was rapidly completed for a garrison of 60 men, which was judged sufficient, and Azambuja himself remained there for two and a half years, by which time the post was prosperous commercially (Cordeiro, 1892). The records of this expedition and settlement are given at length by Barros (1628), who devotes over four pages to a verbatim speech of King Caramancas, expressing his gratitude to the Portuguese for bringing him Christianity and promising him the protection of the Portuguese monarch, but among them we find no record of sickness on which we can rely. A modern chronicler (Cordeiro, 1892, p. 32), indeed, writes: "but the climate began to exercise its deleterious action on that numerous colony, badly disposed to resist it." Yet in his published reference (naturally the unpublished manuscripts to which he refers are not available to us) by a contemporary historian (Resende), no account of any sickness is given.

There was, however, as we have said, much sickness and heavy mortality in the expedition of Santarem and Escobar, which first visited this place in 1471. One is inclined to think that sickness sometimes escaped notice in the Portuguese record of settlements.

Subsequent to its settlement, we find practically no history of this place until it was captured by the Dutch in 1637, a previous attack, in 1625, having failed.

*Angola.* Of the establishments of the Portuguese in Angola, the history is practically all of diplomacy and politics, no colonists going in at first, only the Portuguese diplomatic and commercial agents; the former with necessary escorts. Save that, although unhealthful to Europeans, it was less so than São Thomé, we have no detail of its healthfulness or the reverse in very early times. From the experience of the Dutch, who captured Angola in 1640, it must have been in that interval unhealthful, but not continuously or severely so, as were the island colonies of São Thomé and São Thiago.

*Island colonies.* The Madeiras, uninhabited when found, were settled

under license by Zarco and Texeira in 1424 and 1425. There is no history of any sickness during the settlement, and they have always enjoyed the reputation of being extremely healthful, with an ideal climate. One outbreak only of yellow fever has been reported there, in 1738 (Guyon, 1858, quoting Rodriguez de Avreu).

There is no account of sickness during the settlement of the Cape Verde Islands; and of such as occurred in the Canaries and at São Thomé, accounts will be given in the chapters dealing more fully with these colonies.

#### SUMMARY

As regards historical records giving definitive evidence of the presence of yellow fever, the history of the mainland of West Africa during discovery and settlement—and indeed for many years thereafter—is negative. Yet it should be noted that the negative record is not of the same weight here as in the New World. There we found, in the period of settlement, conditions such that, had yellow fever existed, it would have manifested itself in such way as probably to be recognizable in the records that we have. Moreover, we have, in the epidemic of 1648 in Yucatan, proof that an area well suited for maintenance of the infection must have been previously free from it—at least for a very long time.

In Africa, on the contrary, we have found no conditions under which yellow fever, if present, would have been certainly recognizable in our histories. The records themselves are not so full as for America, and the conditions are such that yellow fever, if it did occur, is not to be distinguished from the ever-present and severe malaria. Thus, we find, in not a few places, accounts of what *may* have been yellow fever in Africa, but the circumstances do not permit certain identification.

A further study of the island colonies may be more fruitful, for unlike the settlements on the mainland, which were small military or trading posts, those on certain of the islands were true colonies, with fairly large numbers of European immigrants. It is true that the islands, except the Canaries, were uninhabited when discovered, and hence do not come into question as places of origin of yellow fever if we believe that this is an infection peculiar to man. However, once settled, the colonies of São Thomé, the Cape Verdes, and the Canaries become of importance as places in which to seek early evidence of yellow fever. *Aedes aegypti*, if not indigenous, must have been introduced early; and the same traffic that brought them would, sooner or later, introduce the infection of yellow fever if it existed on the mainland.



The effect upon the colonists themselves might not be discoverable in such scant records as we have, for, after the first settlements, immigrants came in small numbers at a time; and if infection were frequently introduced it might readily happen that even with a high prevalence of yellow fever no *epidemics* would occur sufficiently extensive to be definitive. There would be more likelihood of discovering the infection by its development on board ships visiting the islands, especially in the larger naval expeditions. It is to the latter, then, that we shall direct our particular attention.

## CHAPTER XVI

### THE ISLAND OF SÃO THOMÉ

#### SICKNESS DURING SETTLEMENT

São Thomé (St. Thomas), a little island some 400 square miles in area, lies on the Equator, less than 200 miles from the mainland of Africa. Uninhabited when discovered, in 1470, it was settled by the King of Portugal. The account that is given of this by a number of writers (Dapper, 1676; Barbot, 1732; Labat, 1731; and Faria e Sousa, 1730—the last being by far the best account) is that at the first attempt to settle it, by João de Payva, in 1485, the settlers all died, or so many died that the attempt to settle it with volunteer immigrants was abandoned and, in 1493, *déportés* were resorted to. These were mainly the children of Jews who had reverted from their forced conversions. So many died, however, of those who went directly to São Thomé, that later the immigrants were first settled on the mainland of Angola. A certain number died here, for the place was not healthful for whites, but far fewer than if they had gone directly to São Thomé, and when these people moved from Angola to the island, they lived there in safety.

This account of the early settlement of São Thomé is concurred in by so many writers that it should either be true or else—as may have happened—all followed the same tradition. It is analogous to the method used by the coffee factors at Maracaibo prior to 1914 for their European personnel. The clerks newly arrived from Europe were sent at once to Cúcuta, where they were subject to a “fever of acclimation” of only moderate severity. Moving, then, to Maracaibo, they were safe from the yellow fever prevailing there, which otherwise was frequently fatal.

It would be, however, an unwarrantable assumption to make the same explanation for the two cases. The accounts quoted witness less that a residence at Angola did protect from the sicknesses at São Thomé than that it was the belief of our chroniclers that it would do so. And even if this belief were true, it does not necessarily imply a *specific* immunization against disease by residence in Angola. It may have been that becoming gradually accustomed to hard and unsanitary conditions of life was less dangerous to these immigrants than to be exposed to them



suddenly. There is an acclimation to living conditions as well as a special acclimation against disease.

These accounts, however, do show the belief that protection from the severe sicknesses of São Thomé was given by residence in the less unhealthful climate of Angola, as the similar belief was later current in Maracaibo which, in the latter case, meant the existence of yellow fever in both Cúcuta and Maracaibo.

#### GROWTH OF THE COLONY

The reputation of São Thomé for unhealthfulness and a peculiarly obnoxious climate continued long after its settlement, but did not prevent its development into a prosperous colony. The earliest contemporary account that we have of it, after its settlement, is that by an unnamed "Portuguese Pilot," addressed to Count Ramon della Torre of Verona, but written, it is said, at the special request of the celebrated physician, Fracastorius. It is translated and published by Ramusio in his *Navigazioni et Viaggi* (1563-83, vol. i), and is given as a nearly complete translation from Ramusio by Walckenaer (1842, vol. i, pp. 376-401).

The writer states that he had made five voyages to São Thomé, the first in 1520. There is no direct statement as to when the document was written, but there is internal evidence of a date about 1535 to 1540. The account is that of a close, intelligent observer, and shows no little native ability in the writer, although he apologizes for being "only a man of the sea and not accustomed to the use of a pen." It is a long document, mainly given, naturally, to maritime conditions, sailing directions, and customs and products of the country, which were new to Europeans, but it contains a surprising amount and variety of information about the island.

The principal product, he says, is sugar. Although only one-third of the island had been cleared, production was already on a large scale—he mentions about sixty sugar-mills using water-power and others using horse- or man-power—and, for its cultivation, negro slaves had been imported in large numbers, some colonists owning as many as two or three hundred. The city of Pavaosan (Pavoasan), now St. Thomas, comprised "six or seven hundred families." One judges this is exclusive of slaves, though it doubtless included mulattoes and possibly negro freemen, some of whom are mentioned as being rich and intelligent. Even with a liberal allowance for slaves and for whites outside of the city, this is still a small population for the maintenance of yellow fever. The account implies, however, that the colony was growing, inducements

being offered for the occupation of uncultivated land. Moreover, the shipping seems to have been active, so that there must have been considerable immigration of transients in ships' crews, and of permanent settlers.

#### SICKNESS PREVALENT AFTER SETTLEMENT

Of the sickness usually prevalent in the colony after its establishment, we have accounts from four observers who visited the island between 1520 and 1725.

"*The Portuguese Pilot.*" The earliest record is that of our Pilot. Concerning the health of São Thomé he says: "In the windy months," May, June, July, and August, "the negroes, unless protected from cold, get sick and die. For Europeans these are the most healthful. In the hot months, December, January, and February . . . the negroes are well and gay . . . the whites are feeble and prostrated. Without having fever positively they feel a fatigue and a general malaise; and can hardly drag themselves along . . . using a cane to walk . . . lose appetite and are always thirsty. . . . All dine together . . . as a change of food from one household to another tempts the appetite. It is too hot to go out and attend to business. . . . The white inhabitants of Pavaosan . . . have an attack (*parosismo*) of fever about every eight to ten days, . . . lasting only for two hours or a little more . . . and to prevent this are careful to be bled three or four times per annum. When the fever takes the newly arrived it is nearly always fatal. This fever for them differs and lasts 20 days and one bleeds the patient up to eleven times and draws from him at each time an entire bowl of blood; . . . The 7th and the 14th days of illness are critical, and if the patient passes these well there is hope for his recovery.

"There is no record that this island has ever been afflicted with a '*pestilentia*,' as has happened to the Cape de Verdes, where it raged one time, it is said, with an extreme violence . . . but the whites are subject to these ardent fevers and to dysentery. . . . Few pass fifty years, while the negroes," thin and healthy, "live to one hundred and ten years."

In addition, he describes smallpox and syphilis, calling the latter (doubtless with Fracastorius' full approval!) "the French disease." He gives, indeed, a very bad account of the excessive heat, moisture, and fogs, and of the illness which they engendered among the white inhabitants.

*Dapper.* The next—though much later—account is by Olfert Dapper, a Fleming, a great traveller in Africa, who visited São Thomé apparently



between 1640 and 1664, and published an account of his travels in Flemish in 1676. He gives a very full account of the island, and I think he had read the Pilot's account in Ramusio.

He says: (*op. cit.*, pp. 68–76) “The hot, intemperate, humid air is harmful, especially to Europeans, who rarely reach 50 years or show white beards. The natives are often over 100.” He adds that young white people cease to grow after they arrive at St. Thomas. He blames this unhealthfulness on the great heat “joined to the vaporous humidity. This is not the same everywhere over the island, but principally over the city” (Pavoasan) “it reigns mixed with some kind of a malignity.”

“The maladies which reign principally are hot and malignant fevers, of which the whites, and above all strangers, feel this attack in a few days, because, although one stops as little as may be in this city to sojourn, yet they feel themselves so suddenly and violently attacked with an ardent fever that very often it carries them off the 4th or the 7th or, at the least, the 14th day. If they pass those days they generally get well, unless they commit some excess of drinking or eating, for the best remedy is to observe a good regimen of living and to eat little and to fast much in order to be able to pass the 14th day. Nevertheless it is necessary to bleed often and to purge.”

He mentions, both in São Thomé and in Angola, on the mainland, a sickness, the “*Bitios de Ku*” as “very common and very dangerous,” “a species of dysentery, presaged by an extraordinary melancholy and accompanied with a great pain in the head; with lassitude and with pain in the eyes.”<sup>1</sup>

*Barbot.* Jean Barbot, “Agent General of the Royal Company of Africa and the Islands of America” (French) who was long resident on this Coast, gives a detailed description of São Thomé, published in Churchill's *Collection of Voyages and Travels* (1732, vol. v). It was written, apparently, soon after 1682.

He speaks of the hot and humid climate, “pernicious to *Europeans*, who can scarce live there to fifty years of age,” although the natives are healthy and many of them “attain to an hundred years of age.” He makes the same complaint as his predecessor of the “thick, stinking fogs” and of their unwholesomeness, such that even “the natives are forced to keep close at home for a time.”

For the distempers, he mentions fevers, smallpox, “cholicks,” the bloody-flux, the venereal disease; and another called there “*Bichos no Cu*,” besides several others, particularly “head-ache.”

<sup>1</sup> For an account of the origin of the name “*o bicho*” or “*a bicha*” among the Portuguese, see page 195, footnote 6.

“Fever, above all other distempers, destroys the greatest number of people, especially new-comers from *Europe*, carrying them off in less than eight days sickness. The first symptoms of it are a cold shivering, attended with an intolerable heat or inflammation in the body for two hours, so as to throw the patient into a violent delirium, which at the fifth or the seventh fit, or the fourteenth at most, makes an end of most persons seized with it; the fit returning every other day. If the patient escapes, he may reasonably expect to live there several years in health, provided he is temperate . . . well dieted after having been purged . . . with *Cassia* infused in the blood of vipers. They allow the patient to drink water plentifully during the fit.”

The “Cholicks are there so terrible, as to distract the patient in three or four days.” They are ascribed to the evening dews, cooling off while sweating, as in the breeze at night, or drinking cocoanut milk, etc.<sup>2</sup>

“This sort of cholick has swept away an incredible number of people. . . .

“The . . . *Bichos no Cu* is also very common there, both among *Whites* and *Blacks*, the nature of it is to melt or dissolve men’s fat inwardly, and to void it by stool. . . . The *French* call this distemper ‘*Gras fondu*,’ that is, melted grease.”

*Labat*. Finally, Père Labat, in his *Voyage du Chevalier Des Marchais* . . . (1731, tom. iii, chap. i) tells of conditions at São Thomé at the time of his visit (1725). Although this account is written after the time limit (1648) we had set, its description of the prevalent diseases is valuable because of Labat’s intelligence and his familiarity with yellow fever. He it was who reported the “*mal de Siam*” introduced into the Antilles by the *Oriflamme* in 1690. He observed it closely at that time—as a priest he had much occasion to see it—and had a severe attack himself.<sup>3</sup>

He gives practically the same account of the sicknesses as did the “Portuguese Pilot” and the other two we have quoted. Indeed, his pathological data are so closely in accordance with the others that one feels quite certain that he must have read Ramusio at any rate, which, indeed, was probable, as he was a learned man, familiar with Italian, and was of great industry.

His description of the “*bitios*” is, however, somewhat more explicit than Dapper’s or Barbot’s. He says (vol. iii, pp. 5–6):

<sup>2</sup> These last three factors are alleged by all our writers as causal. Stanley (1885), too, lays considerable stress on the cooling off as a cause of fever in Africa.

<sup>3</sup> He states that he had two attacks in Martinique. The first, from his description, must have been yellow fever. From the same data it is hard to say what the second was.



“The second sickness is called in Portuguese ‘*Bitios de Cu.*’ It is an ulcer which comes in the lower bowel which gives very acute pains with fever & transport to the brain” (delirium). “This sickness carries away in three or four days those who are attacked of it & corrupts so quickly the blood & the flesh of all the bodies that they fall into rotting before the sick man gives up his spirit. It has been believed for a long time that it comes from a total dissolution of the mass of the blood and an entire coagulation of it. . . . It has been in Brazil for many years. It has passed from Brazil to the islands of America & from there to the *tierra firme*. It has been called, in the French islands, the ‘*Mal de Siam,*’ because it was brought by a vessel of the King . . . the *Ori flamme*, which, returning from Siam after our defeat in that country, had been obliged to refresh in that country” (Brazil) “where it took aboard this plague (*mauvais drogue*).”

He is enthusiastic for and stresses the treatment previously mentioned by Dapper and Barbot—forced lavements of a decoction of cassia and lemon juice, followed by suppositories of pieces of lemon—which, he says, “cures the sick man in a few minutes,” relying, one judges, rather on Portuguese reports than his own observation.

#### INDICATIONS OF YELLOW FEVER ON THE ISLAND

The diseases prevailing at São Thomé, mentioned in the records which we have quoted, were dysentery or “bloody-flux,” fevers, and “*bitios de cu*”—whatever that might be or include. Of these, dysentery need not be discussed; it was too well known to the Europeans of that time. However, if yellow fever were present at São Thomé, it could have been included either among the “fevers,” or—considering the very varied symptomatology given—in the blanket diagnosis of the “*bitios.*” The first epidemic of yellow fever introduced into Brazil (1686) was known under a similar designation, “*epidemia da bicha,*” by the Portuguese.

*Fevers.* Malarial fever was unquestionably prevalent, and likely the most generally prevalent. It is so clearly indicated in the descriptions, from that of the “Portuguese Pilot” on, that there is no need of discussing this question. There are a few points, however, which may imply the co-existence of other fevers therewith. As possibly indicating yellow fever we have:

(1) The statement that the fogs exercised a “special malignancy” in the fevers they produced in the city as compared with the country. This is more like yellow fever than malaria, which is essentially a rural disease.

(2) The statement that if one recovered from an attack of the severe

fever he was apt to go "several years" without another attack. This implies a belief that there was a severe fever, one attack of which gave an immunity against recurrence, and may be taken with the analogous belief concerning the fevers in Angola and São Thomé at the settlement of the latter.

"*Bitios de Cu.*" It is difficult to judge what this represents. Although our reporters seem explicitly to differentiate it from dysentery, one judges quite a number of cases of the latter were included under this designation. Sigaud's (1844) description of the disease later called "*bicho*" in Brazil strongly suggests *schistosomiasis mansoni*, and the therapy which he mentions is similar to that described for the "*bitios*" in São Thomé. It is quite possible, then, that our "*bitios*" here includes schistosome dysentery. However, some parts of the descriptions of "*bitios de cu*" suggest yellow fever, namely:

(1) The acuteness ascribed to it; the high fever, weakness, pain in the head and eyes—the latter especially characteristic of yellow fever and dengue—are by no means characteristic of *schistosomiasis mansoni* or of dysentery. They are characteristic of yellow fever.

(2) The violent abdominal pains ascribed to the "*bitios*" one might naturally consider as identifying it with dysentery—bacillary or amoebic, or from *schistosomiasis*. They might as well, however, be the excruciating epigastric pains accompanying the retching hiccough which precedes black vomit in yellow fever. "Cramps of the belly—very painful," is noted by many of the older writers as characteristic of yellow fever. Certainly, death within four days would not occur with *schistosomiasis* and would happen in yellow fever. Nor is the wild delirium, which is so stressed in our descriptions of the "*bitios*," a symptom of either *schistosomiasis* or dysentery, while it is markedly so of yellow fever.

(3) Labat's account stresses these characteristics and adds another, on which he lays stress, namely, the decomposition of the body before death. While this does not occur in yellow fever, it is worth noting that Labat had stated that the same occurred in the "*mal de Siam*" in Martinique, and that Schotte (1782, p. 131), in his account of the yellow fever in Senegal in 1778, states: "... the inflammation of the liver, of the stomach, and perhaps of the intestines, was changed into a gangrene, and this was followed by an incipient putrefaction of the whole body, and death." One judges that in both cases what was referred to was the large livid infarcts and ecchymoses that show in bad cases of yellow fever, the skin over which sometimes sloughs. Scurvy shows the same appearances, and the older writers characterize it, too, as showing "decomposition" before death.



(4) Labat thought he recognized this "*bitios*" as the "*mal de Siam*" that he had seen in America—which *was* yellow fever. Or, to put it more accurately, he recognized *some* cases of "*mal de Siam*" among the sicknesses designated as "*bitios*" at São Thomé. Certainly, we need not give too much weight to this identification, yet Labat had had much and prolonged experience in yellow fever, and one is apt to note very closely a disease from which he himself has suffered; and it may well be that he did see cases of yellow fever in São Thomé and recognized it by the appearance of the dead and dying, for as a priest he would be especially associated with these.

It was natural enough that there should be no close differentiation of diseases, but that under one nomenclature should be confused severe cases of any malignant fever, along with *schistosomiasis* or other dysentery. The "*bitios*," then, while very probably including some severe cases of bacillary dysentery and possibly some of *schistosomiasis*, may well have included yellow fever also, just as the "*fièvre bilieuse inflammatoire*" of the French Antilles included other things along with yellow fever.

One can not say that anything in the accounts we have quoted establishes the existence of yellow fever here. The most that can be said is that, if yellow fever were present, we should expect the accounts to have been much as they are.

#### SICKNESS IN THE DUTCH FLEETS, 1599 AND 1641

During the years that we have covered, São Thomé was twice captured by the Dutch: first, in 1599, by Admiral Van der Does, and again, in 1641, by Admiral Jol; and both times the invaders suffered extremely heavy losses from sickness. These epidemics are mentioned and, to an extent, described by Dapper, Barbot, and Labat in the accounts from which we have quoted. As they wrote from information which must have come to them at second or third hand, presumably from Portuguese sources, and apparently at least in part from tradition, their reports of the sickness among the Dutch can not be considered very reliable as to symptoms and mortality. Still, they serve at least to confirm the reputation of the island for unhealthfulness.

Of Jol's expedition, Dapper says: "The Dutch had no little number of deaths and of sick men when they made themselves masters of the island in 1641 under Admiral Jol, for there hardly remained 10 or 12 men sound of each Company, and Jol himself died, besides several other chiefs. Finally the malady raged with so much fury and it extended itself so among his men that there hardly remained enough for sentinels, and so

few sailors that there was difficulty in equipping two vessels," which he sent to Count Maurice in Brazil for help. "The most part died of pain in the head so violent that it threw them into madness and folly. Some had a pain in the belly which carried them off in 3 or 4 days."

Barbot, speaking of "this sort of cholick" that "has swept away an incredible number of men," says: . . . . "It . . . . with the bloody flux, kill'd such a number of *Dutch* men the two several times they subdued the island, in the years 1610 (*sic*) and 1641, as before mentioned; that the island was then known in *Holland* by the name of the *Dutch Church-yard*." His account adds nothing of importance, and need not be quoted further.

Père Labat's account is again similar—indeed suspiciously so—to those of his predecessors: "Admiral Yol was master of the fort and the city and of all the flat country, but the sickness carried him away also and with him nearly all the chiefs of his troops and of his fleet and such a great number of soldiers and sailors that there remained scarcely enough to stand sentry and to furnish sailors for two ships, which made them abandon or burn the others."

"The most part died of an ardent, putrid fever, accompanied with a headache so violent that they became foolish and as insane. Some others were tormented with pains in the bowels so excessive that the strongest could hardly bear them to the 4th day. Since this time the *Hollanders* have lost the desire to go to be buried in such a dangerous country."

As regards mortality, these accounts are confirmed from various other sources. Thus, Netscher (1853, p. 121), writing from Dutch sources, says that of the Dutch garrison of 600 men placed on the island in October, 1641, only 230 remained in December, and he also records the death of Jol and many other high officers. The mortality here is high enough for the disease to have been yellow fever; and the symptomatology that we have quoted—scant and traditional—are not inconsistent with that diagnosis. However, the circumstances do not permit exclusion of other diseases or other places as sources of the infections, for before attacking São Thomé the fleet had taken S. Paulo de Loanda and had been on the African coast some two months.

Intermingled with the accounts which Dapper, Barbot, and Labat give of the sickness in Jol's forces are allusions to the epidemic in Van der Does' fleet in 1599, but as to this their data are less definite. Since Van der Does came to São Thomé from the Canaries, the history of his expedition and the sickness therein may be given better in a later chapter dealing with those islands and supplementing the records that we have from São Thomé with data from other sources.



## CHAPTER XVII

### THE CAPE VERDE ISLANDS

The Cape Verde Islands, discovered by Ca da Mosto in 1456, were settled by the Portuguese under royal license in 1460-61. Nothing is said of their health conditions during settlement, but from the quick success of the venture and the absence of any report of sickness, there was probably none of a serious nature at this time. Indeed, the islands having been previously uninhabited, no infection confined to men could have occurred unless brought by the Portuguese settlers.

Later, this group, and especially its principal island, São Thiago, was reported by the seafaring men of many nations as very unhealthful and at times pestilential. This was natural. Because of their location and commercial relations, these islands were peculiarly liable not only to receive any infections, existing on the west coast of Africa, but also to concentrate them and to become a focus for their dissemination among Europeans. The main factors in this were: first, and chiefly, their relation to the African slave trade, and second the use of São Thiago as a port of call.

#### RELATION TO THE AFRICAN SLAVE TRADE

The source of the African slaves was the mainland. Angola and, to a much less extent, São Jorge da Mina were the centers of collection for slaves from Portuguese West Africa. Many, too, came a little later from the Benin district, though this region was not claimed as under Portuguese suzerainty. The ultimate destination of the slaves was principally tropical America; at first to Spanish settlements, and later to those of other nations as they were established.

As only Portuguese ships were allowed to enter the Portuguese ports of the mainland of West Africa, and only Spanish ships were allowed to trade with the Spanish ports of America, obviously legal trade between Portuguese West Africa and Spanish America could not be direct, except during the time from 1581 to 1640, when Portugal and Spain were under the same crown. To provide for this trade the island of São Thiago, in the Cape Verdes, was made an intermediate depot and distributing center. To this place slaves were brought, in Portuguese vessels, from the mainland and, this being a free port (as were those on other Portuguese

islands) from it vessels of other nations carried the slaves purchased here to their American possessions. In the sixteenth century the slaves were carried from São Thiago, mainly by Spanish vessels, to the West Indies.<sup>1</sup>

If, then, yellow fever had been in Portuguese West Africa, São Thiago, from its close communication with the Guinea coast, and from the presence of the crews of many European vessels, would be especially apt to be infected with that disease, and would be more apt to be a source of infection to European vessels, bound for the Americas, than Guinea itself, because the index of infection of the *aegypti* at São Thiago would be kept high by the continued introduction of susceptible men.

The presence of a large number of European vessels here not only would serve, by the presence of their crews, to continue this infection here, but by developing epidemics among them after leaving, would make the disease noticeable; and vessels bound for the tropics would furnish an ideal mechanism for its conveyance to infectible territory.<sup>2</sup>

We have record that in later years slavers were frequently infected with, and conveyed, yellow fever to the New World; and if the depot from which they drew their slaves was infected with that disease in early times, they would unquestionably have done so then.

#### SÃO THIAGO A PORT OF CALL

In addition to being a depot for the slave trade, São Thiago was a port of call between Europe and the New World, universally used by the Portuguese, by the Spanish between 1581 and 1640, and very generally by the ships of all nations. This might, indeed, bring no yellow fever to

<sup>1</sup> There was also a considerable trade in slaves taken from parts of the African coast not under Portuguese control, and carried legally to the French, Dutch, and English colonies in America, also—illegally—to the Spanish colonies, sometimes by connivance of their local authorities, and sometimes with their consent enforced. The Dutch were especially active in this trade during the time they held Angola, 1640 to 1648, taking slaves to the French Antilles and the English colonies as well as to their own. Later, but after the years in which we are interested (i.e. after 1648), the British became the principal carriers.

<sup>2</sup> São Thiago could not be infected with yellow fever unless itself infested with the insect vector. These it would early have received from the Guinea coast by sailing vessel communication if they existed there; and *aegypti* as well as the parasite of yellow fever could have been transported by vessels to the American tropics. The introduction of the infection into places where few or no *aegypti* existed, even in the tropics, would not have led to propagation of *yellow fever* there. São Thiago early depended on rain for its potable water, so that water storage must have provided favorable breeding places for *aegypti*, which at present are abundant there.



it, but would serve both to show ship-borne epidemics and to distribute the disease to other places in the tropics, as would the slave trade.

#### UNHEALTHFULNESS OF SÃO THIAGO

This place, São Thiago, as we have said, held a bad reputation for unhealthfulness among the seamen of all nations. The British Colonial Office (Report of 1920), it is true, states that the Cape Verdes are now generally healthful, which implies that in the past they presented no basic conditions for malaria. The report also states that, although *aegypti* are common, there has been no yellow fever since 1854. Bérenger-Féraud (1890, pp. 139–144), however, reports its presence thereafter in 1862, 1864, and 1868.

That health conditions in times past were not good, is witnessed by many. The "Portuguese Pilot," whose account of São Thomé has been given in the preceding chapter, comments on the freedom of that island from any recorded "*pestilentia*," "as has happened to the Cape de Verdes, where it raged, one time, it is said, with an extreme violence."<sup>3</sup> Sir Richard Hawkins, "The Compleate Seaman," than whom no one was more competent to speak of matters relating to the sea at this time, in his account of his voyage of 1593 to the South Sea (Hawkins, 1622, pp. 27–29) has this to say of the Cape Verdes:

"These islands are held to be scituate in one of the most unhealthiest Climates of the world and therefore it is wisdom to shunne the sight of them, how much more to make abode in them.

"In two times that I have beene in them either cost vs the one halfe of our people with Fevers and Fluxes of sundry kinds . . . and in one of them it cost me six moneths sicknesse with no small hazard of life."

Of the commerce of São Thiago, he says that it has "great trade by reason of the neighbor-hood they haue with Guyne and Bynne" (Guinea and Benin); "but the principall is the buying and selling of Negroes."

Leaving such general statements, we take up particular instances of infections contracted at São Thiago.

#### EXPEDITION OF DRAKE AND CARLEILL, 1585

In 1585 Drake and Carleill sailed from England on September 12, with 2,300 men all told, soldiers and seamen, in twenty-five vessels. On November 16, "in the evening" they landed 1,000 men or more on the

<sup>3</sup> This epidemic Bérenger-Féraud notes as possibly yellow fever. Save that a general and fatal epidemic is indicated, and in the tropics, no reason is seen for doing so. He cites no other evidence than the allusion by the "Portuguese Pilot."

island of São Thiago of the Cape Verdes and took the city of that name with little resistance. They stayed "fourteen days," going inland about twelve miles, and reshipped November 26, setting sail the same evening.

These dates and the statement that they stayed "fourteen days" are taken verbatim from *A Summarie and True Discourse of Sir Francis Drakes West-Indian Voyage, etc.* (Bigges, 1652).<sup>4</sup>

Obviously, landing the evening of November 16, and sailing the 26th, they could have remained on the island only *ten* days. The chronicler (Drake's lieutenant) must have counted the whole time the vessels were among the islands. He is quite certain to have been correct on all the *dates* given, which were doubtless taken from the ship's log, especially as he notes that the day the town was taken was the seventeenth of November, Queen Elizabeth's coronation day, in honor of which the English fired great salvos from the captured guns.

After leaving the islands, the narrative continues: "We were not many days at sea but there began amongst our people such mortality as, in a few days, there were dead above two or three hundred men and untill some seven or eight dayes after our coming from Saint Jago there had not dyed any one man of sicknesse in all the Fleet: the sicknesse shewed not his infection wherewith so many were strocken untill we were departed thence and then seazed our people with extreme hot burning and continuall ague whereof some very few escaped with life and yet those, for the most part, not without great alteration and decay of their wits and strength for a long time after. In some that dyed were plainly shewed the small sports (*sic*) which are often found upon those that be infective with the Plague."

They were eighteen or nineteen days before reaching the island of Dominica, in the West Indies, where they took water. They spent some days in late December at St. Kitts, uninhabited at that time, to rest their sick people; landed 1,000 to 1,200 men on Hispaniola, on New Year's Day, about ten miles west of Isabela; took the city, and stayed there the space of one month, until it was ransomed. They then went to Cartagena, which they captured, and stayed there six weeks, while the ransom was being raised. While at Cartagena, "the sicknesse with mortality before spoken of *still continuing among us*" (italics the writer's) "though not with the same fury as at first," they gave up their original design of going to Nombre de Dios and thence across to Panama.

Our chronicler further adds of this outbreak: "such as were touched

<sup>4</sup> See also Hakluyt, Goldsmid ed., Edinburgh, 1885-90, vol. xv, pp. 208-239.



with the said Sicknesse escaping death very few, or almost none could recover their strength, yea many of them were much decayed in their memory; in so much that it was growne an ordinary judgement, when one was heard to speak foolishly to say he had beene sicke of the Calentour, which is the Spanish name of their burning Ague; for, as I told you before, it is a very burning and pestilent Ague. The originall cause thereof, is imputed to the evening or first night ayre, which they tearme *La serena*, wherein they say and hold very firme opinion, that who so is then abroad in the open ayre, shall certainly be infected to the death, not being of the *Indian* or naturall race of those Countrey People: by holding their Watch, were thus subjected to the infectious ayre, which at *S. Jago* was most dangerous and deadly of all other places.”

The fleet returned to England via St. Augustine, which they captured and burned, thence by Roanoke Island, where they took off Lane's starving colony, arriving at Portsmouth July 28, 1586. No sickness is recorded on the return trip. On the whole voyage they had lost “in all 750 men, counting those killed.”

For identification of the “Calentour,” we have that it was: (1) a disease of high mortality, (2) developing and prevailing aboard ships on the high seas, and (3) picked up—or developing soon after leaving—a port in the tropics notoriously unhealthful.<sup>5</sup> Of these three things we are certain, and they are characteristic, even if not distinctive, of yellow fever. One must, however, consider in addition to yellow fever, the possibility of *scurvy*, *smallpox*, *typhus*, *plague*, and *malaria*. Nothing else had been liable to occur in such an epidemic aboard ship or to have been picked up here.

*Scurvy* would not have occurred with severity so soon after leaving port, where the men had certainly refreshed themselves.

*Smallpox* and *dysentery*, being well known, had certainly been named.

For *plague*, as sudden and great an explosion had been impossible, except in the pneumonic form, and this form of plague is not a disease of hot climates, such as that of São Thiago. Moreover, the mortality of pneumonic plague, in modern times, is practically 100 per cent; and had this been the cause of the epidemic, there had been little mention of men convalescent from it. Also, one would think, pneumonic plague, in a fore-castle, would have killed every person who stayed in it, and been

<sup>5</sup> There had been no deaths from sickness in the expedition until seven or eight days after leaving São Thiago.

quickly over. So, spite of the small spots they found on the dead, one thinks this disease may be excluded.<sup>6</sup>

*Typhus.* The climate of São Thiago being distinctly tropical, and the subsequent voyage entirely in the tropics, any lice-borne disease would seem to be excluded, nor do we know of any epidemic of typhus fever ever reported in the Cape Verdes, although it not unfrequently prevailed in Spain and Portugal and must have been, from time to time, introduced into the islands.

*Malaria.* There was likely enough malaria at São Thiago, and it might well have occurred among the men who were ashore there—about 1,000—but only among them. The mortality reported, however, which would be about 20 to 30 per cent of the men reported to have been ashore, is entirely too high to be characteristic of a single infection with malaria. Note, too, that the deaths occurred in a short space of time, as if they were from a single attack of acute disease, not in men worn out from recurrences, and that the figure we have is for community mortality, not case mortality. All that we have for case mortality is that “few escaped with life.” The high mortality and the sudden outbreak are clearly characteristic of yellow fever: not necessarily distinctive of it, however.

The only description given, save that it was a “burning ague” and that convalescence was slow, with mental debility, is of the small spots (*petechiae*) on the dead. Slow convalescence is not characteristic of yellow fever, but *petechiae* are found in this disease as well as in typhus, and indeed much more commonly in it than in plague. They occur spontaneously in bad cases, and every bite of a mosquito or other insect on a patient with severe yellow fever would show as a petechial spot. Schotte (1782), in his description of what we are sure was yellow fever, in Senegal, refers to *petechiae* as characteristic, and so prominent that the name “*febris pulicaris*” had been proposed.

The writer has just noted certain data given in this account as characteristic, even if not distinctive, of yellow fever. He thinks that when, to these three points, we add (4) the elimination of the diseases which he has shown should be eliminated, and (5) the presence of *petechiae*, the evidence, while maybe not sufficient to establish this sickness as yellow fever, does strongly indicate it. Certainly he thinks it renders yellow fever a

<sup>6</sup> During the present pandemic of plague, which has lasted over thirty years, the writer can find no instance of an epidemic of plague of any kind aboard a ship on the high seas, although there have been instances reported of a moderate number of cases—all bubonic—aboard ships in Chinese waters.



more probable diagnosis than any of the others that could reasonably be assumed.

If we knew how long the disease continued aboard ship and to what degree, if any, it occurred among those who had not been ashore; or whether it spread from ship to ship, we would have a better chance to determine its nature. If one only had the logs of these vessels, giving the names of those who were sick, the dates of going off and returning to duty or dying, we would know whether or not there were recurrent attacks, how long the invalidism of those who recovered lasted, and the duration of the sickness to death—data which should enable us to determine absolutely, even without any description, whether the sickness was yellow fever or not.

Our chronicler believes that the fever which occurred at Cartagena and—implicitly—up to the time that they left that place, was the same as the one contracted at São Thiago over three and a half, or maybe four and a half, months previously. This, if we accept it as fact, would eliminate everything but yellow fever and, indeed, yellow fever itself would scarcely have lasted so long save by rather slow transference from ship to ship. One cannot think, however, that this statement can be taken at its face value. Any fever occurring in Cartagena, unless extremely different in its symptomatology from that which had prevailed earlier, at sea, would be almost inevitably regarded as the same.

There is no direct mention of sickness during the month the force stayed at Hispaniola, nor indeed at any time from the first outbreak until the occurrence at Cartagena. Yet the language of the narrative, “still continuing among us,” strongly implies that it had persisted. Note too: (1) Not only was malaria usually prevalent around Cartagena, but they stayed there six weeks; and the implication is (it is only an implication, of course) that, at first, the sickness was not serious enough to influence their plans. (2) It is recorded that the Cartagena fever “was not with the same fury as at first;” and this difference in severity is shown by the statement that of the first outbreak on the ship, “very few escaped with life” and made a slow convalescence; while of the second, at Cartagena, it is said of those escaping death, that “very few, or almost none could recover their strength.” In other words, the first sickness had a high mortality and slow convalescence; the second one had a slow convalescence, but nothing is said of its mortality, except that it was not so great as that of the first.

These two accounts would fit in much better with the supposition that yellow fever was taken on at São Thiago, eliminating itself in these small

collections of men, each ship being in itself, to some extent, a unit; and that, at Cartagena, the men contracted malaria, a disease sufficiently like yellow fever to have been quite naturally not differentiated from the first epidemic—assuming that to have been yellow fever.

#### SICKNESS IN OTHER EXPEDITIONS

There are examples of other European expeditions visiting São Thiago during the Brazilian War between the Portuguese and Hollanders, beginning in 1624, when the Dutch Admiral Heyn captured Bahia, and ending in 1654.

Two expeditions were sent out by the Portuguese during this war: one in 1624 and one in 1638, both of which stopped at São Thiago, and both developed at this place (or soon after leaving it) epidemics of greater or less extent and severity.

*Menezes.* In 1624 and 1625 the section of the Portuguese Ocean Fleet under Menezes, 4,000 men in 26 vessels, waiting between fifty and sixty days at São Thiago as port of call from Lisbon to Brazil, contracted a “contagion” there, from which it suffered severely. (Southey, 1817–22, vol. i, pp. 489–490).

From the statements given that a pest arose in Menezes’ fleet while waiting here and that “he lost many men in the fatal climate of the Cape de Verdes,” we do not know whether or not this sickness persisted aboard the ships during the voyage to Brazil. It may all, or nearly all, have occurred during the fifty or sixty days he was waiting at the islands and not have been in any sense a ship-borne disease. Its occurrence is mentioned simply to show that the islands were generally unhealthful or pestilential.

*Mascarhenas*, in 1638–39, with 7,000 men in 26 galleons and 20 other vessels, proceeding to Brazil via the Cape Verde Islands, where the fleet assembled, had the same unfortunate experience as Menezes, losing en route “one third” of his force, both soldiers and seamen, from a pestilence apparently contracted in the Cape Verdes. The sickness continued aboard ship until they reached Brazil, so that they were too weak to attack Pernambuco, as intended, and it was necessary to proceed to Bahia “as to an hospital” (Southey, 1817–22, vol. i, p. 650).<sup>7</sup>

<sup>7</sup> Of this expedition, William Piso (1648, *lib. ii, cap. xviii*) says: “*Anno 1639 magno nostrorum commodo, juxta Sinum Omnium sanctorum tertia pars classis Hispanicae maligno (sic) et contagiosa febre extincta est.*” The epidemic is noted also by Baerle (1660) and da Rocha Pitta (1730); and Béranger-Féraud (1890, p. 28) cites Ferreira da Rosa as giving an account of it. Da Rosa being a physician, the



Mascarhenas stayed no great time at these islands—the writer cannot find exactly how long—but the sickness continued aboard ship during the entire voyage to Brazil and quite certainly afterwards, since he went from Pernambuco to Bahia “as to an hospital.” The date of his leaving the Cape Verdes is not known, nor the time occupied in the passage, but the recorded times of four expeditions made about this time—by Pieter Heyn, 1624; Menezes, 1624; Adriansen, 1630; and Andrada, 1685—give about forty-two days as the average for this voyage.

Except that we have no description of the sickness, the data presented for this epidemic are strikingly like those for that in Drake’s expedition of 1585. As in the latter, we have here a ship-borne disease of high mortality; contracted at the same island; lasting aboard ship in this case certainly during the entire voyage to Brazil, which was probably about forty-three days, to which may be added the time from Pernambuco to Bahia, usually about fourteen days. The writer is inclined to apply, indeed feels justified in applying, the same analysis to this as to Drake’s epidemic, and considers that the data are strongly indicative of yellow fever.

Bérenger-Féraud (1890, p. 28), who accepts the sickness in Mascarhenas’ fleet as probably yellow fever, thinks that it was likely introduced by the fleet into Brazil, and that it prevailed in 1640 in Pernambuco (where Mascarhenas did *not* land), and elsewhere “in Brazil” in 1643. He quotes to this effect, M’Kinlay and Lepoix, but an examination of these references shows no sufficient reason for the belief. More than forty years later, in 1686, an epidemic of yellow fever, severe and general, prevailed in both sections of Pernambuco, Recife and Olinda, and in Bahia, where Mascarhenas had landed his sick (da Rocha Pitta, 1730). It was recorded in both places as an absolutely *new* disease, and apparently in a community virgin to it. Yellow fever might, of course, have been introduced into Bahia by Mascarhenas (I cannot see how he had any opportunity to do so in Pernambuco), but unless the town was then sufficiently infested with *Aedes aegypti*, the disease could not have established itself. That no yellow fever showed in Brazil prior to 1686, notwithstanding its long traffic with the Cape Verdes and the west coast of Africa, may well have been due to there having been

---

writer hoped to get some data on this disease other than was given by laymen. So far as he knows, however, the only book ascribed to da Rosa is the *Trattado Unico da Constituiçam Pestilencial de Pernambuco* (Lisbon, 1694). Positively no note of it is in this book. Had da Rosa identified the sickness in the fleet or even compared it with the one treated of in this book, which was yellow fever, we might have had some useful data.

no general infestation by *Aedes aegypti* in the earlier years of this communication.

EDITORS' NOTE: The author's notes for this portion of his manuscript include data on two other naval expeditions which suffered fatal epidemics after stopping at the Cape Verde Islands. These are the fleets of Baldwin Hendricksz, sent by the Dutch to Bahia in 1625, and that of Gomez Freyre de Andrada, proceeding, in 1685, from Lisbon to Maranham. Concerning Hendricksz' expedition, the references given (Southey, de Laet, and Brito Freire), with others that have been available, leave in doubt what communication, if any, the fleet had with the Cape Verde Islands. In Andrada's expedition, the history of a stop at these islands and of fatal sickness at sea thereafter is sufficiently definite, but satisfactory details of the nature of the sickness, its time of occurrence, duration, and mortality have not been found. Moreover, the date of this expedition is well beyond the limit (1648) of the author's inquiry. It has, therefore, seemed unnecessary to include the incomplete accounts of these expeditions, as the author considered the data inconclusive.



## CHAPTER XVIII

### THE CANARY ISLANDS AND VAN DER DOES' EXPEDITION

#### DISCOVERY AND SETTLEMENT

The Canaries, believed to be the half-mythological "Fortunate Isles" of the ancients, are said to have been visited, about 40 B. C., by Juba of Mauretania and, in the twelfth century, by Arab traders, but they remained unknown to the Europeans until discovered—or rediscovered—in the fourteenth century. They were then inhabited by a white race known as the Guanches, who were in the Stone-Age period of culture. They seem to have been an exceptionally fine people. They embraced Christianity readily, absorbed the culture of the Spaniards, and a number of them are recorded as being in ecclesiastical and legal positions, and some even as hidalgoes of Spain.

The Spanish occupation of the islands was begun in 1402 by Jean de Béthencourt, Sieur de Caux, and Gadifer de la Salle, both Normans, and was completed in 1495 by Alonzo de Lugo, who conquered Teneriffe, the last of the islands to be subdued.

There was a very severe epidemic in Teneriffe during its conquest, in 1494–95 (Espinosa; also Nuñez de la Peña). This "*modorra*" is accounted by Finlay (1904) as probably yellow fever, imported from Santo Domingo by the vessels which Columbus sent back with Torres, in March, 1494. Raging among a wild, pastoral people in the mountains, and sparing the Spaniards in a fortified camp at a river's mouth, it could not have been yellow fever. It was quite surely some disease imported from Spain, and to which the Spaniards were immune by previous attack, as they paid no attention whatever to this sickness and evidently had no fear of it. Even then, without absolutely accepting the statement of good Father Nuñez de la Peña (p. 153) that "not one Christian nor any citizen of the Kingdom of Guimar, in which dwelt the Most Holy Image" (of Our Lady of Candelaria) "was touched by this pestilence," we can be quite sure that the Spaniards at least escaped it and considered themselves immune.

The name, "*modorra*," given to the sickness by the Spaniards, would imply that it was probably typhus, then endemic in Spain, although measles and perhaps influenza are also possibilities. One of these would

account better than typhus for the rapid spread of the epidemic. Small-pox, one thinks, had been mentioned by name and at least feared by the Spaniards. It was not yellow fever.

#### TRADE AND COMMUNICATIONS

Before the "sixty years' captivity," 1581 to 1640, inclusive, when Portugal was under the Spanish crown, there was no direct legal trade between the Canaries and the Portuguese possessions on the African mainland, hence there was little chance for infections to be brought directly from that coast to the islands. There was, however, some communication between the Canaries and the Cape Verdes, and there was importation of African slaves from São Thiago by Spanish vessels into the Canaries. Thus, prior to 1581, the main—perhaps the only—chance of infections coming from the West African coast to the Canaries, was indirectly through São Thiago, that port being liable to infection from Guinea and likely itself to infect the Canaries.

From 1581 to 1640, however, all legal restrictions against Portuguese commerce with the Canaries were removed, so that Portuguese as well as Spanish vessels could then carry slaves to these islands, either from São Thiago or directly from the Guinea coast.

In addition, the Canaries were used as ports of call, especially, but not exclusively, for Spanish vessels between Africa and the Peninsula. Gran Canaria, the principal port, was larger, had a better food supply, and was more healthy than São Thiago, hence was more used by the vessels of both nations.

This communication and the fact that the Canaries had a very considerable population susceptible to yellow fever—not only the inhabitants, Guanches and Spaniards, but also the crews of European ships stopping there—would, during this time, have rendered these islands, especially the principal ones, Gran Canaria and Teneriffe, liable to receive the infection of yellow fever if it existed on the African coast and to propagate it, for a time at least, when received. There were the same reasons for this during this time as those we have given for the same thing in São Thiago, both for the introduction and the retention of the infection. The Canaries were less close to the Guinea coast than the Cape Verdes and, on account of Spanish law, could not be visited by foreign vessels, but the susceptible population was very much larger than at São Thiago.<sup>1</sup>

<sup>1</sup> In the eighteenth and nineteenth centuries yellow fever, when introduced into the Canaries, has spread, giving rise to several severe epidemics, but has not con-



## THE EXPEDITION OF VAN DER DOES, 1599

In the summer of 1599 the Dutch Admiral Pieter Van der Does attacked and captured the island of Gran Canaria. Thereafter, with one half of his fleet, he sailed south. We have three sources of information on this expedition.

(1) An Englishman who accompanied the fleet as master of a victualler was interviewed, after his return to England, in the fall of the same year, by Hakluyt, who gives us, in some detail, the proceedings in the Canaries.

(2) Dr. G. F. Pop, getting his data from the Royal Archives of Holland, has given a brief account of the sickness that prevailed in the fleet after leaving the Canaries.

(3) Three visitors to São Thomé, between 1640 and 1725, give some data, derived, one judges, from local records and tradition, on the sickness among the Dutch after they arrived there.

*Hakluyt's narrative*

According to Hakluyt, (1885-90, vol. vi, pp. 183-204) this fleet of 73 vessels of war, with 8,000 soldiers and seamen, anchored off the island of Gran Canaria June 26, 1599. The castle was carried by storm, in which Van der Does received several wounds. The city was entered on June 29 and plundered, the inhabitants having abandoned it the day before. The town was burnt and the castle blown up July 4, and the Dutch went aboard ship the next day, that is after six days ashore and three days additional in the harbor. They went ashore again July 9, and stayed until the 15th. This last landing seems to have been at the island of Gomara, on which they plundered a small town. They finally weighed anchor and left July 21, one half sailing back to Holland under Admiral Gebransten, with whom went Hakluyt's informant, reaching the Texel September 10.

---

tinued (recognizably) in any island longer than the second year. According to Bérenger-Féraud, citing for the earlier occurrences Busto y Blanco, whose work (*Topografía Médica de las Islas Canarias*, Seville, 1864) has not been available to us, epidemics of yellow fever occurred in the Canaries in 1701, 1771-72, 1810-11, 1862, and 1888. Bérenger-Féraud thinks it possible that earlier epidemics, reported as "*peste*" in 1512, 1531, 1582, 1599, 1601, and 1606, may have been yellow fever, but no reasons are given for this suspicion. The epidemics of 1701 and later may be accepted as authentic, and their occurrence as *epidemics* implies, of course, that during this period yellow fever was *not endemic* on the islands. This inference does not necessarily extend, however, to earlier years.

This record is contemporary, and the data were undoubtedly taken from the ship's log. Hence, the *dates* are reliable.

*Pop's account*

Pop (1867, p. 126), a physician, writing of the history of the medical service of the Dutch Navy, using records in the Royal Archives from 1597, says:

"The first important *epidemic sickness* among sailors of which we find mention in these records, is that which occurred in 1599 in Admiral van der Does' fleet. Including the soldiers there were 8,000 men on board the 73 vessels of the fleet. The purpose of this expedition, the destruction of the Spanish naval force at Coruña, miscarried, because the Spaniards received timely warning of its approach. With half of the ships the course was now continued to the Canary Islands and thence to St. Thomas. On the passage across the Tropic of Cancer an unknown, contagious, and pestilential sickness broke out, of which, on the Admiral's ship alone, 15 people died before the passing of the Line" (Equator). "It was a burning and raging fever, called '*calentura*' by the Spaniards, caused by an alteration of the atmosphere, because the latitude was very ill-adapted to the constitutions of the sailors, especially because of the incessant rain which caused great foulness, stink, and worms on the ships. Sailing (on) to St. Thomas there developed again a severe sickness which was called scurvy ('*scheurbuik*').<sup>2</sup> But while the ships were lying at the island, the sickness in the men increased to such extent that 1,000 sailors and the Commander-in-chief died there. On the homeward journey, which was undertaken speedily, the sickness and mortality continued; nor were the officers spared, for all but two of the ships' captains died. When we recall that the Spaniards, at St. Domingo, at that time called yellow fever *calentura*—which name had been adopted by the English who, under Drake, had suffered severely from yellow fever—then we may conclude that yellow fever prevailed in Admiral van der Does' fleet, and was contracted on the Canary islands, where the seamen had been some time ashore. . . . That the sickness which arose sailing to St. Thomas was scurvy, may well be; but lying off the shore of this island a high mortality from this disease is not to be explained. For on the English ships at St. Thomas the sufferers from scurvy were quickly cured by the use of lemons, as was the case with the crews

<sup>2</sup> This is given by Pop as a quotation "from a manuscript in the files of the expedition (Royal Archives)."



of the *Canterbury* and the *Norwich*.<sup>3</sup> There is, then, more ground for the belief that the Dutch fleet was affected on that island with severe malarial fever which was then endemic there. Indeed, the malarial infection there was so severe that the resulting fevers had become proverbial and were considered the most malignant in Africa or America, while Lind calls quinine the only remedy for them. We need not be surprised then, to find that even on the homeward journey these seamen, already so reduced in health, continued to suffer from severe illness."

Of the destination of that half of the fleet which did not return to Holland, Hakluyt's victualler—who did not go beyond the Canaries, and was hardly in the Admiral's confidence—says they sailed "for the West Indians." Pop says merely "to St. Thomas," and Bérenger-Féraud (1890, p. 28), citing Pop, interprets this as meaning "St. Thomas of the Antilles." Possibly he was misled by Pop's allusion to the experience of British ships at St. Thomas—which does refer to "St. Thomas of the Antilles," but to an occurrence more than 100 years later. Whatever its origin, the inference is wrong, for it is quite certain that the fleet went not to the West Indies but to the island of St. Thomas (São Thomé) off the coast of Africa.

#### *Accounts of Dapper, Barbot, and Labat*

As a third source of information about the sickness in this fleet, we have the reports written by Dapper (1676), Barbot (1732), and Labat (1731), the three travellers whose descriptions of São Thomé have been given in a preceding chapter. All of these mention Van der Does' capture of the island and the great mortality which his forces suffered there. It is a question how much reliance can be placed upon their accounts. They wrote from 50 to 125 years after the event, and we do not know from what sources they drew. They all make a mistake of eleven years in the date of the raid, which they give as 1610 instead of 1599, and all give Van der Does' name as "Verdoes," obvious errors which do not speak well for their accuracy in other details. The uniformity of the reports is, we think, evidence not of reliability, but rather of their having used a common source or copied one from the other.

Dapper, whose report of the epidemic that afflicted Admiral Jol's forces when he captured São Thomé in 1641, has already been given

<sup>3</sup> The allusion here is to an account which Lind, in his *Treatise on the Scurvy* (1757, 2nd. ed., p. 155), gives of the experience of two British warships, arriving "at the Danish island of *St. Thomas*," in the West Indies. The date, not given, is apparently in the first half of the eighteenth century.

(see p. 229), says: "This poisoned air caused a mortality greater yet among those whom Admiral Pieter Verdoes commanded, when, in the month of November, 1610, he took possession of this island with all of his fleet, for there died in less than 15 days more than 1,000 people, all of this sickness, endemic and peculiar to this country, which principally made itself known by pains in the head and of the belly. The Admiral himself died, and Vice-Admiral Storm, besides 17 Captains of vessels and all of those of the land army save one. The fury of this general and contagious sickness went finally so far and made such great ravages that bodies were opened." The only appearance noted seems to have been that the intra-abdominal fat was melted and fluid, and hence the name "*gras fondu*" (melted fat) was given to this illness.

Barbot, speaking of the diseases prevalent on the island, says: "Cholicks are there so terrible as to distract the patient in three or four days." . . . . "This sort of cholick has swept away an incredible number of people. . . . It . . . . with the bloody-flux, killed such a number of *Dutch* men the two several times they subdued the island, in the years 1610, and 1641 . . . . that the island was then known in *Holland* by the name of the *Dutch Churchyard* . . . . The first expedition was under Admiral *Pieter Verdoes* or *Vander Does*, in the month of *November*, 1610, with seventeen men of war . . . . he stormed the town and carry'd it. . . . . Soon after the country distempers seized them in such manner that above a thousand soldiers died; as did also the admiral *Verdoes*, his vice-admiral *Storm*, and all the officers of the land-forces, except one only."

Labat's account is similar: "Fort San Sebastian resisted in 1610 the army of the *Hollanders* . . . . commanded by the Admiral *Pierre Verdoes*, who made himself master of the city. . . . The bravery of the Portuguese Governor and of his garrison was powerfully aided by the maladies of which we have spoken previously, which, in less than fifteen days, carried away the Admiral; Vice-Admiral; 17 captains of vessels and several other officers and the greater part of the troops which had been put ashore, of which all the Captains perished except one. All the rest would have had the same fate if they had persisted in this enterprise. It was necessary then to abandon it and to return to *Holland* with the debris of this unhappy fleet."

Labat adds this description of the sickness among the Dutch, but whether it refers solely to the epidemic in Jol's forces in 1641, or also to that in Van der Does' fleet, is not altogether clear from the context. "The most part died of an ardent, putrid fever, accompanied with a headache so violent that they became foolish and as insane. Some



others were tormented with pains in the bowels so excessive that the strongest could hardly bear them to the 4th day."

The data which we have from the several sources we have quoted are:

(1) The entire fleet of 73 vessels was in or near the Canary Islands from June 26 to July 21, and at least a part of the men were ashore there for some days.

(2) Leaving the Canaries, one half the fleet returned to Holland, and the other half, under Van der Does, went to São Thomé. The half that returned to Holland made a slow voyage, but there is no mention of sickness among them.

(3) In the half of the fleet that went to São Thomé an epidemic broke out while crossing the Tropic of Cancer, of which 15 men on the Commander-in-chief's ship died before the Equator was crossed.

(4) Later there was very much more sickness, which caused great mortality at São Thomé, and—according to Pop—on the homeward voyage.

There are several questions here.

(1) What was the nature of the epidemic which appeared when the fleet reached the Tropic of Cancer, and of which fifteen men died on the Admiral's ship before the passing of the Equator? Except that it was "a burning, raging fever, called by the Spaniards '*calentura*,'" there is no description of this disease on the flagship, and extremely little epidemiological data. As Gran Canaria is, at most, not over 270 miles in a direct line from the Tropic of Cancer, where the sickness "broke out," I think we can exclude scurvy; the time after leaving land seems too short. The men, those of the Admiral's ship especially, from the prominent part he bore in the attack, were quite certainly ashore and had a chance of eating green things and fresh meat at Gran Canaria, where they stayed in all at least nine days, besides six or more at Gomara.

The low latitude and the fact that the time was midsummer should, one thinks, rule out typhus, which, too, would scarcely have been called an "unknown malady," as it was a common disease of the north European countries, both ashore and aboard ship. The same reasons hold for rejecting plague as are given for so doing in Drake's expedition. None of the Canaries have ever been regarded as malarious, and it scarcely seems reasonable to suppose that Van der Does would have picked up enough malaria there, in so short a stay ashore, as to have caused 15 deaths aboard a single ship.

Whether the sickness at this time was on any other ship than the Admiral's we do not know, nor is the number of her crew given; but the *Revenge*, Drake's flagship in the fight with the Armada, and hence

clearly a ship of the first class, had 150 men as her full crew in her last fight in the Azores, in 1591. Unless the Dutchman, then, had a very much larger crew, the loss is too heavy to have been caused by malaria, aboard ship on the high seas, from a single short exposure ashore, even in a badly infected port—which Gran Canaria was not.<sup>4</sup>

Pop accepts this as yellow fever, and such facts as he gives point in this direction—the sudden outbreak at sea of an acute fever, “unknown, contagious and pestilential,” and with a high mortality. There had been time, too, for the development of secondary cases of this disease from any primary infections contracted at Gran Canaria. We have, however, very little data.

(2) Where and under what conditions occurred the epidemic called “*scheurbuik*” (scurvy); and was this the same as that which had caused the 15 deaths on the Admiral’s ship? The manuscript that Pop quotes says the epidemic of “*scheurbuik*” broke out “sailing to St. Thomas,” which would make it no great time after the “*calentura*.” Whether or not Pop is right in thinking that some scurvy might have occurred on this voyage, he is unquestionably right in declining to accept it as causing the great mortality at São Thomé. This had been impossible.

(3) What was the nature of the sickness that caused such mortality at São Thomé and on the homeward voyage? Clearly it was some disease or diseases either brought from the Canaries or contracted at São Thomé. If brought from the Canaries—an extension of the same epidemic that had caused the 15 deaths on the Admiral’s ship—it could only be yellow fever. Nothing else likely to be contracted there would have continued so long aboard ship and grown into such a fatal epidemic. We have, however, no sufficient ground for believing that the sickness at São Thomé was a continuation of that which had occurred en route from the Canaries, for this is only a possible, not at all a necessary, interpretation of Pop’s record, while the accounts of Dapper, Barbot, and Labat all clearly imply that the sickness among the Dutch was contracted at São Thomé—or was at least so considered—and that it was thought to be a disease “endemic and peculiar to this country.”

Of the infections prevalent on the island, we need to consider only those sufficiently acute and fatal to account for the mortality ashore in the time the Dutch were there—apparently not more than a few weeks—and at the same time likely to have continued aboard ship.

<sup>4</sup> Netscher (1853) gives the complement of each ship in several Dutch fleets assembled between 1620 and 1650. The largest number mentioned on any one ship is 262—155 seamen and 107 soldiers.



It is true, as we have said more than once, that a sickness of high mortality carried a considerable time aboard ship on the high seas in the tropics—if we exclude cholera, of which there is no question in this part of Africa at this time—is apt to be yellow fever or scurvy. Nevertheless, some of the conditions here are not quite the same as those we have hitherto encountered in ship-borne epidemics, and we must consider as possibilities some other diseases, especially malaria and dysentery.

(1) *Malaria*, naturally, would not be contracted aboard ship, except when lying close inshore, nor would it generally be apt to last very long once on the high seas, or with heavy mortality. These men, however, were exposed for a considerable time. The accounts from Portuguese sources imply that they were ashore over 15 days, and it probably was considerably over that, and a large number going aboard with a heavy infection might well make much sickness which, by recurrences in the men originally infected, might last for a considerable time and cause a number of deaths. Undoubtedly malaria was highly prevalent at São Thomé, and we would expect it to have caused a good deal of sickness and some deaths. Whether it would be sufficient to account for such mortality as Pop's account indicates is a question. To judge of this one would need to know more of *when* the deaths occurred—how many ashore, how many at sea, and in what part of the voyage.

(2) *Dysentery*, of bacillary origin, may well have been an important factor in the epidemic ashore: it is so indicated in Barbot's account. It is not considered a ship-borne disease, yet if a large number of men thus infected were aboard a crowded vessel, there might be propagation of it. One would think, however, that dysentery, well known to the Dutch, would have been called by name, even if this had been a more severe type than that with which the Hollanders were familiar. Amoebic dysentery is slow in evolution and could hardly account for much sickness, certainly not for many deaths before the fleet reached Holland, unless this trip was unusually prolonged. The same applies, with even more force, to *schistosomiasis mansoni*.

(3) *The relapsing fever of the tropics*—a tick-borne spirochetosis—one judges could not have been propagated aboard ship or, if so, for only a very short time and in a minor degree. Certainly the writer has been able to find no instance of a ship-borne epidemic of this disease, and he has looked for it. There are reports from the West African Coast, of a number of spirochetoses, said to be different from relapsing fever and yellow fever, although the writer doubts their certain differentiation from the former. There is no evidence that they exist, or

have existed, at São Thomé, although this is possible enough. We know little of them except that none of them have ever been reported as ship-borne diseases.

(4) *Yellow fever*, if present in São Thomé, would have produced an epidemic. Some of the men would have been infected from the infective *aegypti* already present in the city; but there would be no great epidemic until secondary cases developed from the men thus infected—that is at least three weeks after they had landed. Brought aboard ships, the infection would have spread and continued a long time, until checked by exhaustion of the human material or by cold weather.

(5) *Scurvy*. The record which Pop quotes calls the epidemic which prevailed at São Thomé and on the homeward journey “*scheurbuik*”—scurvy; but the use of this name is, we think, without significance as indicating the presence of scurvy aboard. It is given to a sickness said to have prevailed at São Thomé, and scurvy had naturally been absolutely impossible to men ashore, or with easy access to shore, on a well populated and prosperous tropical island. It might have occurred on the homeward voyage; yet, as unquestionably the fleet reprovisioned at São Thomé, unless this voyage was unusually and excessively protracted, scurvy could not have caused much sickness and many deaths during it. It is possible, indeed, that what was called “*scheurbuik*” may have been yellow fever. The *course of sickness* in yellow fever and scurvy is absolutely different, yet the *appearance of those dead or near dying* of these diseases is not dissimilar, so that one who knew scurvy only from description (or even from having seen the dead bodies) might well mistake between them. Indeed, even at much later dates many eminent writers (Bruce, 1759; Pringle, 1810; Arejula, 1806; and others), influenced, it seems, by the appearances of the sick and dead<sup>5</sup> believed that yellow fever and scurvy were at least kindred diseases.

The Dutch ship-surgeons undoubtedly had knowledge of scurvy, but not necessarily at first-hand, for this disease is said to have been less prevalent on the Dutch ships than on those of other nations—due, perhaps, as Lind thought, to their inclusion of pickles in the ships’ stores. If the knowledge which the physicians of the fleet had of scurvy was from the descriptions given in lectures and text-books, naturally these

<sup>5</sup> The writer himself, not unfamiliar with yellow fever, when he saw the first dead body submitted to autopsy at Ocean Springs, Mississippi, in 1897, remarked: “It looks bad, but it *may* be a jaundiced man with scurvy.” He confesses that he was extremely loath to accept the diagnosis of yellow fever if it was honestly possible to reject it, which it was not.



would stress the appearances of *bad* cases, the bleeding gums, the livid spots and ecchymoses under the skin. These are the very appearances most prominent in bad and fatal cases of yellow fever.

It is not utterly improbable, then, that the name "*scheurbuik*" was given this epidemic because the appearances of *bad* cases of yellow fever—and especially of the dead—corresponded to the *description* given of the same class of cases in scurvy. One can conceive no other reason for its use, as the epidemic was clearly not one of scurvy.

As we have said, one naturally expects a disease of high mortality prevailing aboard ship in the tropics to be either scurvy or yellow fever. Scurvy is, we believe, absolutely excluded here, and while in this case, as we have said, other infections are possible, we think we can claim that none that could reasonably be expected to be aboard so well satisfies all the data that we have as yellow fever.

As in the case of Drake's expedition, that of Van der Does, while not absolutely indicating yellow fever, *does* indicate it. Strongly on the way from Gran Canaria, much more strongly at São Thomé.<sup>6</sup>

<sup>6</sup> NOTE BY THE EDITORS: In the discussion which has been presented above, important considerations in judging the nature of the disease or diseases which affected Van der Does' forces are the time of occurrence, duration, and extent of mortality *at sea*, before reaching St. Thomas, and again after leaving there; also the length of the stay on the island. Since none of the accounts quoted is sufficiently explicit on these points, further particulars of this expedition have been sought from such sources as have been accessible, especially in the hope of finding significant dates. The results, though meagre, indicate that, in certain respects, the circumstances were not quite as indicated by, or naturally inferred from, the reports cited by Dr. Carter, especially as regards the duration of the voyage from the Canaries to St. Thomas and thence to Holland.

The fullest accounts that we have found are those given by Bor (1621-34, book 36, fol. 41-44) and Meteren (1614, book 21, fol. 435-436), Dutch historians who were contemporary with Van der Does and whose works are cited by modern historians as being authoritative for that period. Except that Bor's account is the more detailed, the two agree very closely. Both confirm Hakluyt's report of proceedings in the Canaries and the dates that he gives, also his statement that on July 21, after the raid on Gomara, the fleet was divided, 35 ships sailing back to Holland under Gebranstén, and 36 remaining with Van der Does. No sickness is mentioned in the half of the fleet which returned to Holland, arriving there in September.

The movements of the other half of the fleet are given by Bor in a letter from Van der Does to the States-General, written off Cape "Lopez Gonsalves" (on the African mainland, nearly opposite St. Thomas) and dated October 13, 1599. He states that leaving the Canaries July 21, he proceeded to the harbor of Maio, (a barren sparsely populated island in the Cape Verde group), anchored there August 4, and remained some days, taking on water. Leaving there on or before August 15,

he decided to raid the Portuguese possessions on the coast of "Guinea and Manicongo" and adjacent islands. There is mention of some Spanish and Portuguese merchant ships captured, but none of any raid carried out ashore up to the date of the letter. No sickness in the fleet is mentioned. On the contrary, he says "all the officers of this fleet (except Captain Mota, who has died) are well disposed thank God." If there had indeed been an epidemic causing fifteen deaths on his own ship, as Pop reports, it is strange that he does not report it. The only mention of sickness is in the announcement of his intention to attack St. Thomas "notwithstanding the unwholesomeness of the air there and along this coast, which still is not so black as it is painted."

Presumably, St. Thomas was reached within the next day or two, for the next date given is that of Van der Does' death, on the island, October 24, and this was evidently some days after landing. After his death, his successor, Storm von Wena, considered fortifying the island and holding it for Holland, but gave up the project when the engineers reported that it would take more than ten weeks—probably, too, because of the prevalence of sickness, this being implied by the return to Holland, for it had been Van der Does' intention to make other raids as far as Brazil. The date of leaving the island is not given, nor the length of the stay there. Leaving St. Thomas, the fleet returned direct to Holland, arriving there in February, 1600, with the exception of six or seven ships which were sent from St. Thomas to Brazil, reaching Holland in June, 1600.

Of the sickness which developed on the island, Bor says: "And as they had now been long at sea and engaged in these exploits, and because the air in this country is very unwholesome for our people, and because our people in this hot country could not well regulate themselves in eating and drinking, and avoiding the fruits of the land; there arose among the people a sickness which is there called *Calentura* or *la Modorra*, which overcomes the people coming there from Europe: It melted the fat in their bodies, like melted butter lying in their bellies."

There died, he says, fifteen captains of land forces and sea forces, and twelve hundred men, both soldiers and sailors. He does not say *when* these deaths occurred, though the context refers to events on the island. However, Meteren, who gives the same figures, says that the mortality occurred "mostly at sea in the ships, on the way home." Wagenaar (1782, book 33, pp. 55–56), writing later, says a thousand died in fifteen days at sea. He, however, represents the whole fleet as proceeding from St. Thomas to Brazil.

On the homeward journey, Bor says, "coming to the height of the 45th and 46th degrees into cold air, the sudden change again caused many sicknesses, especially of *Scheurbuyck*, of which and of other sicknesses several hundred died," including General Storm von Wena, the date of whose death is given as February 8. Meteren does not specifically mention this later sickness, which Bor definitely differentiates from that previously mentioned. Both relate that for lack of men to handle the ships there was great difficulty in reaching port, so that one ship had to be abandoned, and others put into English ports, while the rest reached Holland with most of those on board sick.

These accounts, which would seem to be reliable as far as they go, if accepted, would modify Dr. Carter's discussion in several respects, namely:

(1) If any epidemic occurred between the Canaries and St. Thomas, it must have been of limited extent, and, if yellow fever, must have been confined to one



or a few ships. Unless the manuscript which Pop quotes from the Royal Archives is the record of an eye-witness, one is justified in discounting it, in view of Van der Does' failure to mention an epidemic before reaching St. Thomas.

(2) The outbreak of sickness with great debility, on reaching cold latitudes, on the homeward voyage, would indicate malaria, recurring in men previously infected, and, since the voyage may have been of three and a half or four months' duration, scurvy is not excluded.

(3) The statements of Meteren and Wagenaar, that most of the deaths occurred at sea, imply that this was in the earlier part of the voyage. If this is so, and especially if it is true that 1,000 died in fifteen days, as Wagenaar says, the evidence for yellow fever is much strengthened.

Since there is uncertainty about essential facts, there is, of course, no certainty about deductions from them, unless the requisite additional facts are available from other and entirely authentic sources.

## CHAPTER XIX

### SICKNESS IN AFRICA CONSIDERED TO BE CERTAINLY YELLOW FEVER

In the accounts that we have presented, we have found records of sickness on the mainland of West Africa that *may* have been yellow fever, especially in the commercial voyages of the sixteenth century. In the islands off the coast, and in the fleets that visited them, we have found, even before 1600, what we think was very probably yellow fever. But neither in the records that we have quoted, nor in such others as we have examined, have we been able to find, prior to 1648, when yellow fever is identified in America, any sickness in Africa which, from its description, *must* be accepted as *certainly* yellow fever. For recognition of this disease in Africa as certainly as it is identified for us in Cogolludo's history of the epidemic of 1648 in Yucatan, we must pass over another hundred years and more, to the reports which Lind and Schotte give of the fevers in Senegal and Gambia. Of these two writers, only Schotte gives a description that is entirely definitive, but as Lind's reports are very commonly quoted in the literature of yellow fever, we will consider them first.

#### LIND

James Lind, a distinguished authority of his day on tropical diseases, on scurvy, and on naval hygiene, gives, in his *Essay on Diseases Incidental to Europeans in Hot Climates* (1792),<sup>1</sup> descriptions of several outbreaks which have been accepted as yellow fever—some erroneously, and some, we think, without sufficient proof. There is, however, one account in which yellow fever is very strongly, if not quite certainly, indicated.

In August, 1768, the sloop *Merlin*, with a complement of about 90 men, entered the Gambia river. "She continued six days in the river, employed in wooding and watering. While there, all the men were in

<sup>1</sup> This is the fifth edition of this treatise. The first edition, published in 1768, naturally does not contain the account of the fevers on the *Merlin* in 1768 and on the *Weasel* and the *Hound* in 1769. The writer has not been able to examine the intermediate editions.



perfect health, but in about two days after they put to sea, those who had been employed in wooding were successively taken ill; afterwards those who had been employed in the duty of watering, were seized in the same manner. At first these men alone were seized with the fever, and several of them a day continued to fall sick for six or seven days; at length almost all that had been employed on those services were ill; after them, their attendants were seized with the fever, and in such numbers as to leave no doubt of the disease being infectious" (*op. cit.*, pp. 184-5).

This allows an incubation of from two to eight days for the first of the men ashore who fell sick, and of not less than six or seven for the last. Considering that the time during which the cases occurred is given only approximately, as "six or seven days," not exactly, the intervals correspond reasonably to the known period of incubation of yellow fever in man. The interval of time between the onset of the first cases on the *Merlin* and the first cases that occurred in their attendants is not given: it had been to us a valuable datum. We would like to know, too, how many of those that had been ashore sickened, and how many of those who had not been ashore, and how long before the last case occurred on board.

The fact that no one else aboard the *Merlin* contracted this fever until after the last case among those who had been ashore, is evidence—not proof—that men staying aboard the vessel did not contract the fever while she was up the river. Its appearance later in such men does give reason for the acceptance of Lind's opinion that the disease contracted ashore and brought aboard, was "infectious," that is, communicable. The attendants on the sick in the sick-bay would be most, and first, exposed to *aegypti* infected therein—especially if the vessel were under way at sea, as those insects that left the sick-bay would most likely be blown outboard.

No clinical description of the fever is given, so the evidence for its being yellow fever is entirely epidemiological—its occurrence first in men who had been ashore, and its apparent propagation aboard ship. But while yellow fever is strongly indicated, the evidence is not complete. For one thing, we are not absolutely certain of the facts, since Lind here is not reporting his own observation. He does not give the source of his information, but as he was at that time in England, it is supposed that he is quoting the medical officer of the *Merlin*, and presumably from an oral report, since no document is mentioned. Had he followed Hakluyt, in taking his data from the ship's log and giving dates, his report had been more valuable to us. Then, too, there were only four deaths, a

low mortality for yellow fever if the cases were as many as are implied and if all were of that disease. It may be, however, that some were malaria. If so, they would not have been differentiated in the reports, for Lind, familiar with yellow fever at least from the writings of those who had seen it in America, was of the opinion that it was a more malignant form of malarial fever.

Coupled with his account of the fever on the *Merlin*, Lind reports (p. 186) what he evidently considers the same fever on the *Weasel* and the *Hound*, on the same coast the next year, 1769. For these outbreaks he gives no epidemiological data that are helpful, but of the cases on the *Weasel* a full clinical report is given, quoted from the journal of the ship's surgeon (pp. 55-60). Of the ten deaths, four occurred on the eighth day, one on the tenth, and five from the eleventh to the fourteenth day. Since most yellow fever cases die on the seventh day, and the great majority not later than the eighth day, a disease in which one half died after ten days is excluded from consideration as such. Moreover, those not dying are, as a rule, out of danger—save from accident—by the end of the fifth day, which also makes against this diagnosis for the cases on the *Weasel*, where very bad cases had their crises on the fifteenth and eighteenth days.

It is in this treatise that Lind makes the valuable observation, made earlier by Pliny and Lancisi, that malarial fevers were practically not contracted during the daytime. For it he quotes the following (pp. 221-225): In 1766 the *Phoenix*, with 280 men, lay at São Thomé twenty-seven days, and the men were almost all ashore at one time or another during the daytime, but only sixteen stayed ashore at night—some for twelve nights and some, apparently, not so long. All of these contracted fever, of which thirteen died, the other three being left in very ill health. None of the others on board had the fever. The next year the same ship, on the coast of Guinea, lost eight out of ten men “who had imprudently remained all night on shore,” while the rest of the ship's company, ashore only in the daytime, escaped. On board the *Hound*, which accompanied the *Phoenix*, there were no deaths (save one, from an illness contracted in England), “the officers having been particularly careful not to permit any of the people to continue all night on shore.”

The conclusion—for which Lind is in no wise responsible—that these fevers were yellow fever, led to the heresy of the safety of “daylight communication” with places infected with yellow fever, a belief, as the writer has said before, responsible for a large number of epidemics and deaths aboard ships lying off such places, whose personnel had, indeed,



been safer had they come ashore at night than as allowed, and perfectly safe only had they stayed aboard.

## SCHOTTE

Schotte arrived in Senegal in 1775, and in 1782 published a report which had been presented to the Royal Society in 1780, on the "*Synochus Atrabiliosa*" as he had observed it there in 1778.

The place of the epidemic was St. Louis de Senegal. The garrison, he says, had been fairly healthy until early in August, when the epidemic began, "raging" from that time until about the middle of September. Of 92 white inhabitants at St. Louis, 59 died, the deaths occurring from three to five a day during the height of the epidemic, in the latter part of August and the first of September. A similar epidemic was said to have prevailed earlier in the French settlement on the island of Gorée, and Schotte thinks the infection was imported thence, though there seems to have been but little communication between the two places. These epidemiological data, though scant, agree with the facts that we know of the fairly numerous epidemics of yellow fever that have since occurred in the same place, and the clinical description that Schotte gives (p. 41 *et seq.*) leaves no reasonable doubt as to the nature of the disease.

"A constant and uninterrupted fever attended the disease, from the beginning to the end, in all those who died; and in some who recovered, no apyrexia took place before the seventh day, or later; in others sooner." The fever was . . . . "a continued one. . . . Most of those, who were seized with the disease, felt, just before it made its attack, a *languor* and a giddiness of the head. This was soon followed by a *rigor*, which in some was but slight, and in most patients did not last longer than a quarter of an hour; . . . . While this *rigor* lasted, the muscles of the whole body, but particularly those of the face, were slightly convulsed, not unlike a trembling or twitching." The premonitory languor was not always felt. "They all complained of . . . . *nausea* at the stomach, and soon after threw up its contents, sometimes mixed with bile. Now the *rigor* subsided; the body became hot, and the face red. The pulse grew full and quick, but rather soft. . . ." In some, however, the pulse was hard, with signs of a "peripneumony." "The eyes were red and shining, and seemed to project from their orbits. . . . Most patients complained of a great headache, and of a pain in their back, particularly about the region of the loins, and sometimes in their arms and legs. They felt a most acute pain above and across their eyes. . . . Notwithstanding the fullness of the pulse, there

appeared in some a dejection and lowness of spirits, with a failure of strength . . . . All complained of a load, pain, and heat about the *Praecordia*, but particularly about the pit of the stomach. The vomiting of yellow bile now took place, and was often repeated. This gave no relief to the patient, nor did the retching cease, though the stomach was quite evacuated, but a convulsive motion took place, and continued the retching, though nothing was thrown up. This caused a great thirst, which could not be quenched by any kind of liquid; for whatever was drank, was immediately rejected. . . . . Some few did not vomit, nor had hardly any sickness at the stomach . . . . The vomiting continued, and the bile, which was before thrown up of a yellow colour, and in a liquid state, was now changed both in colour and substance. It became green, brown, and at last black, and was coagulated in small lumps. A limpid fluid, not unlike *saliva*, was ejected at the same time, in which the lumps of coagulated bile floated. The bile did not communicate any colour to this liquid, nor did it dissolve in it, but like a greasy matter swam mostly on its surface.

“The pulse was now not so full as before, but quicker; most patients complained of a burning heat within the pit of the stomach, attended with an unquenchable thirst. Some patients, in hopes of finding some relief against this internal heat and burning, stole from the beds, and laid down naked on the floor. Some were seized with a pain in their throat and a difficulty of swallowing, commonly attended with a redness on the outside. Those, who were afflicted with this symptom, died suddenly, and sooner than one would have expected from the other symptoms. A continual *Diarrhoea*, with gripings, now took place, by which a great quantity of black and putrid *faeces* were evacuated. . .

“Those, who survived the third or fourth day, were still afflicted with some new symptoms. The skin became now full of *petechiae*; they made their first appearance about the eye-lids, and on the wrists, and soon after on all the other parts of the body; but the breast was generally most beset with them. They were mostly of a florid red colour, but sometimes purple or livid; they appeared first like small points, and increased gradually into spots of about a line, or a line and a half in diameter, yet they were not circular, but irregular in their circumference.<sup>2</sup>

“Many patients were now seized with a *coma* . . . . *Vibices* also appeared in different parts of the body, but commonly in such, as had been

<sup>2</sup> The *petechiae* were so invariable in appearance that some, he says, had proposed to call the disease “*morbis pulicaris*,” because the spots resembled fleabites, which, however, he was sure they were not.



squeezed or compressed by some means or other, wherefore, they mostly arose on that side or part, on which the sick lay. They appeared just a few minutes before death itself put an end to the horrid scene."

He mentions the spitting of blood from the mouth, which, he says, "seemed to proceed from the gums, the root of the tongue, and the *fauces*." He notes also the suppression of urine, the extreme restlessness and, in the early stages, the sleeplessness, also the remission of pains as the disease progressed. Yellowness is not mentioned in his formal clinical history, but is noted later in his differential diagnosis.

His description differs from the classical one of yellow fever only in his statement of a prolonged convalescence and the occurrence of "relapses." He says: (p. 64) "The few who recovered of the disease . . . were still very weak for a long time, and much subject to relapses of the same disease, of which (when again seized with it) they mostly died. But in this case they were not carried off so suddenly, as those, who died of the first attack. They seldom died before the eleventh day, and some not before the fourteenth. Neither were the symptoms so violent. They generally lay comatose, covered with *petechiae*." This, however, was in a country where malarial infection must have been practically universal. A number of his cases seem, too, to have had diarrhea in the latter stages, and we do not know how much debility may have been due to the therapy employed—severe purging, emetics, and bleeding—which, however, he abandoned later.

He bases his differential diagnosis from the "bilious" fevers of the books on the greater severity of the symptoms, the hemorrhages, the black vomit, and the *petechiae*, and derives his name, "*Synochus atrabiliosa*," from the black vomit, as did the Spanish earlier in the New World, when they called it "*vómito prieto*."

"In the beginning it is not to be distinguished from the fever, which is called *bilious*," (malarial intermittent) "or from that one, which goes by the name of *yellow fever*," (malarial remittent) "but by the severity of its symptoms. For in those the symptoms are the same, but milder, and I am persuaded, that our disease only differs in malignity from those fevers, which I conceive to originate from the same causes, but proportionably less deleterious. In its progress it is not so difficult to be distinguished from them; for the bilious fever has generally intermissions, and the yellow one slight remissions; yet sometimes it happens, that the first of those continues in hot countries to the third day, before it intermits, and that the latter has hardly any remissions on the first days. I also believe the bilious fever to be contagious sometimes, but I have

observed the yellow fever to be much more so. The voiding of black bile upwards and downwards is almost the only symptom in which our disease differs from the yellow fever. The *petechiae*, hemorrhages, and some other symptoms, which do not attend the bilious, are common in the yellow fever. But it is to be observed, that the *petechiae* are less in number in the yellow fever, than in our disease, and in proportion as the skin is more yellow, the *petechiae* are fewer. The skin being mostly of a lurid colour in the disease, I am describing, makes it differ also from the yellow fever. Some medical gentlemen have informed me, that the yellow fever was sometimes attended with the vomiting of black bile," (cases of true yellow fever probably) "but during a residence of four years at Senegal, I had many opportunities of seeing patients in this disease, and never observed it; yet notwithstanding this I do not in the least doubt it, because I think the greatest affinity exists between our disease and the yellow fever, and I believe, as I have already said, that they only differ in the degree of malignity. The *diagnosis* was more easily formed, after the disease had raged for some time. For when healthy and strong people, who either had been waiting on the sick, or visited them, were suddenly taken ill with a similarity of symptoms, it was easily conjectured, that they had received the disease from contagion, and from thence it was probable, that all those, who might be taken in the same manner, would labour under the same disease, and that it was contagious" (pp. 65-67).

"Those fevers rage more or less every year, but I have observed, that they are more or less malignant, in proportion as the weather is more or less rainy . . . but when the rains are heavy and lasting, putrid fevers are the consequence. This remark stands confirmed by twenty years experience of Mr. Bishopp, surgeon to the province of Senegambia. . . ." (p. 110).

Schotte, too, mentions (p. 131) what Labat, in Martinique, had stressed for the "*mal de Siam*," and in São Thomé for the "*bitios*." "The symptoms become gradually worse. The inflammation of the liver, of the stomach, and perhaps of the intestines, was changed into a gangrene, and this was followed by an incipient putrefaction of the whole body, and death."

It is interesting as well as rare to find that he discovered no infallible remedy. He is, indeed, quite definite as to the ill effects from bleeding, purgatives, and emetics, which he had tried at first, but speaks guardedly of the efficacy of the treatment which he later adopted, opium to restrain the vomiting, and "the bark" when it could be retained.



He was a man absolutely honest, a close and critical observer, with a truly scientific mind, learned, indeed, but scarcely abreast of his own times in his learning. His paper is full of quotations from Celsus, Cispalpinus, Galen, and a Latin translation of Hippocrates. If he had only read Towne, Bruce, Griffith Hughes of the Barbados, or Henry Warren of Jamaica, all of whom had published in London before this time, he would have known better and earlier with what he was dealing. Yet it may be that, had he known of these writings, he would not have left us his own so careful and excellent description of what he saw.

#### YELLOW FEVER, WHEN DESCRIBED IN AFRICA, NOT CONSIDERED A NEW DISEASE

In Africa, as in America, it is, naturally, the occurrence of yellow fever in an *epidemic* that furnishes us the earliest description by which we can recognize this disease; but the circumstances and the identifying data are different in the two countries. In Yucatan and the French Antilles, the epidemics of 1648 are definitely recorded as the introduction of a new disease, hitherto unknown in those countries. In Senegal, though Schotte gives a distinctive name, and makes a *clinical* differentiation between the "*Synochus atrabiliosa*" and the usual fevers of the country, there is no indication that he considers it a disease newly introduced. Rather he thinks the epidemic of 1778 is an increase in prevalence and malignancy of fevers already present in the country, especially in 1766 and other very wet years. This he gives on the authority of Mr. Bishopp, "head of the medical establishment in the Province of Senegambia," and with twenty years' experience there.

Note, too, that in Yucatan the indigenes contracted the fever and died of it as readily as the Europeans—or more so—while Schotte reports nothing of the kind in Senegal. Unlike the epidemic in Yucatan, which is definitely recorded as the introduction of a new disease into a susceptible community, that in Senegal seems rather the result of introducing a body of susceptible people into a region already infected with yellow fever. It is more like the epidemics in garrisons, which later became so common in tropical America, after yellow fever had become certainly endemic there.

#### OCCURRENCE OF YELLOW FEVER IN AFRICA AFTER 1778

After the Senegal epidemic of 1778, the next note<sup>3</sup> that we have of yellow fever on the African coast, is of its occurrence in Bulam. This is,

<sup>3</sup> Bérenger-Féraud (1890, pp. 56-57) says that yellow fever was epidemic among the French in Senegal in 1779 as well as 1778. He gives no particulars, except

indeed, identified for us not by observations in Africa, but by the descriptions which Chisolm (1801) and others give of the infection introduced into Grenada, in 1793, by a ship, the *Hankey*, from Bulam. In Grenada, and in other places in the Lesser Antilles to which it spread, it was recognized as being not the paludal or "yellow" fever of the West Indies, but a "new disease," which, from its origin—correctly ascribed in this instance—was known as "the fever of Bulam."

From this time to the present the reports of yellow fever in West Africa, from Senegal to Angola, inclusive, are frequent enough to justify the belief that the infection has been constantly present on the continent. This does not mean that the disease has been reported every year within this area. On the contrary, there are sometimes intervals of several years between successive records.<sup>4</sup> Nor is it intended to imply that there has been no importation of yellow fever from America to the African coast within this time. This has occurred in a fair number of instances where the facts are known and clear, and doubtless at other times unrecognized. Still, yellow fever has been reported in this part of Africa often enough and under conditions in which importation from places outside this area could not reasonably be accused, to justify, the writer thinks, the statement that it has been *constantly* present. It is not meant that any one place has necessarily been infected continuously during the whole of this time, but that there has been no time in which *some* place was not infected. That is, the west coast of Africa, between the limits of Senegal and Angola, has been, the writer believes, a permanent regional focus ever since the disease was first recognized there, and, as he thinks, for a much longer time.

The reports of yellow fever in Africa have been of its existence at European settlements or aboard vessels touching at this coast. It seems to have been more often reported from Sierra Leone than from any other

---

that the French in Senegal suffered a heavy mortality in 1779, and gives no reference. That the infection persisted beyond the first year is, of course, altogether probable.

<sup>4</sup> Béranger-Féraud, in his chronological summary, which agrees in general with that of Hirsch (1883, vol. i, p. 334), notes no occurrence of yellow fever on the African mainland from 1792 until 1815, though his note of an epidemic on the island of Maio, in 1807, supposedly introduced from Guinea, implies a belief in its existence on the coast then. From 1815 to 1889, when his summary ends, he notes the recorded presence of yellow fever on the mainland of Africa in 35 of the 75 years. His record is a careful one, and, so far as the writer knows, is the best available for this period; but obviously, the absence of a published report for any year does not imply actual freedom from the disease.



section of its size, doubtless due to the larger number of Europeans there and the considerable number of British merchantmen and men-o'-war that frequented this region and which, receiving infection there, carried it to a number of places in Africa, as to Gambia, Senegal, the Cape Verdes, Ascencion, St. Helena, and also to some ports in Great Britain, forming foci of infection in the African, but not in the British ports. It has been, I think, rarely if ever mentioned in the older records as occurring among the natives except—and then only occasionally—when associated with an epidemic among the whites; the general teaching having been that the native blacks were not susceptible to the disease.<sup>5</sup>

This is by no means the same as asserting that the infection did not occur among the blacks in former times when it was not reported at all, or that, in recent times, its occurrence among them has been as rare as its reporting. Indeed, it must be obvious that the number of whites on this coast, in early times especially, has not been sufficient to prevent the elimination of the disease by failure of the human host if continuation of the infection depended upon them alone.<sup>6</sup> The reasonable belief is that, during this time, the native blacks were continuously having yellow fever, in first one place of this region and then another, but that it simply was not noticed—or was rarely noticed—among them.

If it seems unreasonable that yellow fever could prevail generation after generation in the negroes of Africa, without being recognized, one need but recall how rare was its recognition in even the white natives of endemic centers in America—this, too, under medical observation closer by far than we have had even in recent years for the great bulk of the blacks of Africa. Take into account with this the racial resistance of the negro to yellow fever, and it is not surprising that in Africa the disease should almost invariably escape recognition in the black race. It would be surprising were it otherwise.

That the negro does possess a resistance to yellow fever not shown by any other race is, we think, among the best established facts in the epi-

<sup>5</sup> Some, as Bérenger-Féraud, holding this insusceptibility dependent upon their remaining in the regions of their birth; others that it is effective independent of locality.

<sup>6</sup> This inference is, of course, dependent upon the epidemiology of yellow fever in the Americas and Africa being the same. If, in Africa, some lower animal serves as a permanent reservoir host, the deduction would not, of necessity, hold. There is strong evidence against the existence of any such lower animal host in America, where, when the supply of susceptible men has been depleted, the disease has disappeared permanently or until reintroduced from without. This evidence against a lower animal reservoir host is lacking in Africa, though we have, indeed, no evidence to indicate the existence there of any host other than man.

demiology of the disease. It is a point on which opinion is all but unanimous and evidence conclusive. Of course the negro, just as does the white man, acquires a *specific* immunity from an attack of the disease, but it is certain that he has also a true racial resistance which is not dependent upon prior infection or exposure. This has been shown in many epidemics in the southern part of the United States where negroes, living in areas that had long been free from yellow fever, suffered much less severely than the whites when epidemics occurred. We have, too, the experience of the Kordofan half-brigade sent to Mexico in the French expedition of 1862, as related by Dutroulau (1868, p. 465), Cornilliac (1886, p. 453), Corre (1882, p. 36), and others. There had been no yellow fever in Kordofan in historic times, yet these troops enjoyed an "almost absolute immunity," and suffered no deaths from that disease, while the rest of the expedition, similarly circumstanced, suffered severely.

Note, however, that what we know of the negro's reaction to yellow fever is that he is resistant to its *toxins*. We have no knowledge that he is resistant to the *infection*. He seems to contract yellow fever, it may be, as readily as do whites,<sup>7</sup> but to have it far less severely and with much less mortality.

Thus, although in times past yellow fever has practically never been reported in West Africa except when it occurred in connection with a white settlement, the current belief—and the one which is shared by the writer—is that the disease was, and is generally, propagated by the negroes, especially by the negro children. As reasons for its non-recognition in the negro we have:

(1) The mild and unmarked reaction characteristic of the negro.

(2) If it occurs chiefly in black children, the disease in them would be still milder and more difficult to recognize than in negro adults, because yellow fever tends always to be lighter in childhood—and the adults would be largely immunized.

(3) Sickness among the blacks not associated with Europeans, unless of a marked and severe nature, would not come to the notice of skilled European physicians likely to recognize the disease.

The key to the propagation of yellow fever in Africa is, then, we believe, that it is propagated mainly in the natives, especially in native children, but is noted only when susceptible whites are exposed to the infection. The whites, being too few to account for continued propagation of the disease, serve as indicators of its presence among the blacks.

<sup>7</sup> The Pasteur Commission, working in Brazil (Marchoux, Salimbeni, and Simond, 1903) found that *Stegomyia* fed on negroes less readily than on whites. Two experiments by the writer tend to confirm this, but neither, nor both, are sufficient to establish it.



## CHAPTER XX

### THE PLACE OF ORIGIN OF YELLOW FEVER: SUMMARY

The question that has been discussed throughout these pages is: What is the place of origin of yellow fever? This is not necessarily the place where the disease can first be identified by direct and positive evidence. Indeed, when we are first able to recognize yellow fever with certainty from a record of direct observation—in Yucatan in 1648—it is in a place where we are sure it had been introduced recently, certainly after a long period of freedom, and probably for the first time. To trace the origin of yellow fever further back than its actual recognition, we must depend upon evidence which is less direct—inference from what we know of its epidemiology and from historical records which, without necessarily referring directly to the disease, may imply either its presence or its absence. By such evidence, it should be possible to identify the region in which yellow fever existed earliest in the period covered by such historical records and archaeological data as serve to indicate to us approximately the climatic and sociological conditions existing in different parts of the world. Whether or not the place where yellow fever is first located within these limits of time is the same place where it first evolved as a disease of man, is another question. Our knowledge of the evolution of man and his parasites is too limited for its discussion.

As evidence bearing on the place of origin of yellow fever in the sense that has been indicated, we have given, first, a summary of its epidemiology—the facts that are known of its natural occurrence and its experimental production. From these data is derived the postulate: For the naturally continued existence of yellow fever in any place, there are necessary three factors, coexistent at that place: (1) the causative organism of the disease; (2) functionally active *Aedes* (*Stegomyia*) *aegypti* mosquitoes; and (3) men susceptible to the infection—the mosquitoes having access to both the other two. Subject to the modifications that have been indicated elsewhere with respect to the possible agency of other mosquitoes and the possible existence of some lower animal host in Africa, these conditions are definitely established. Note, too, that while yellow fever can occur and propagate for a time wherever

these three factors exist even temporarily, it can continue only where all three factors are continuously coexistent.<sup>1</sup>

The conditions necessary for the continuous functional activity of the insect vector are known with some exactness; and we know in a general way—though much less exactly—the sociological conditions necessary to provide a continuous supply of susceptible men sufficient to keep up the infection. Hence, we need consider as possible places of origin of yellow fever only those regions where the indicated climatic and sociological conditions have existed in the time with which we are concerned.

Within the part of the world thus delimited, there are areas, comprising parts of East Africa and Southern Asia, where *aegypti* are present in abundance and functionally active throughout the year, and where the supply of susceptible men is sufficient, we think, for the permanent endemic maintenance of yellow fever, but in which this disease has never been known to be established in historic times. At present, so far as we know, the only factor lacking is the causative organism. In the past the insect vector may also have been absent. Be that as it may, yellow fever has not been established in these regions within historic times; and as our history here goes far back, and is such as would have revealed yellow fever if it had been present there, we may safely exclude this territory as a place of origin for the disease.

This leaves only two regions in which climatic and sociological conditions have been suitable for the maintenance of yellow fever and in which it has been known to be endemic—tropical America and West Africa. In one or both of these regions yellow fever must have existed before—and long before—their discovery by Europeans, and if it can be shown that in either one it did *not* exist when the Europeans came, the other becomes the necessary alternative as our place of origin. In either country the positive evidence which we may expect to find of yellow fever is of its occurrence in susceptible people coming into the country; and the chance of recognizing the disease if it occurred will depend upon the conditions under which susceptibles were introduced and the completeness of the records which we have of their experience. Failing, in either country, to find positive evidence, it will be necessary

<sup>1</sup> The presence of the causative organism must be absolutely continuous: the other factors can be lacking for very brief periods. In the absence of functionally active insect vectors or susceptible men, the limit of time necessary for the elimination of the causative organism is the time it can survive in the host it is occupying, without transference. The maximum time is the life of an infected mosquito.



to judge how much weight is to be given to this as negative evidence, showing absence of the disease.

In America, the regions that come into question are: the *tierra caliente* of Mexico, of which we may take Vera Cruz as the center; the Maya country in Yucatan; and the Carib country, including Hispaniola and other islands, and the mainland along the Isthmus of Panama. For all this country we have excellent European records of its discovery, its occupation by Europeans, and its history subsequent to occupation, and for some of it we have native records of a sort, covering the time both before and after the Conquest. The circumstances of European occupation—the way in which susceptible masses were introduced—were such that if yellow fever were already present in America it should have manifested itself in such way as would enable us to recognize it in historical records which noted epidemics and described them even crudely. The European and Aztec records of epidemics in Mexico, which were certainly not yellow fever, have been examined and discussed at some length, for the purpose of forming an idea of their *negative* value, that is, the significance of their *failure* to indicate epidemics of yellow fever earlier than we find them recorded. We think these records are such as would have shown the presence of yellow fever during the early days of the Spanish Conquest had the disease then existed in America.

Of the *tierra caliente* of Mexico, it is found that in the early years of its settlement by Europeans it was certainly unhealthful, but nothing is found in its history which enables us to identify yellow fever. The tradition of devastating epidemics, requiring periodic repopulation of this country—the “*Cocoliztle*” of Herrera—is without certain basis; moreover, it ascribes to the traditional disease an effect—depopulation—which is wholly unlike that of endemic yellow fever. As evidence against the existence of yellow fever here before the Conquest, we have the experience of the Europeans during their first invasion, and the absence of any note in the Aztec records. Moreover, the endemic prevalence of yellow fever here would imply its endemicity also in Yucatan, against which there is very strong evidence.

In the Maya country of Yucatan climatic and sociological conditions at the time of the Spanish Conquest, and long before, were undoubtedly suitable for the endemicity of yellow fever. Several Maya inscriptions referring to “*xekik*” (“blood-vomit”), the name known to have been used later for yellow fever, have been regarded as evidence that the disease had been long familiar to these people. It is not established, however, that the inscriptions actually refer to any event earlier than the epidemic

of 1648, which certainly was yellow fever, or the Mexican epidemic of 1545, which was *not* yellow fever but was characterized by a "pushing out" of blood. For negative evidence in this country, we have unusually full records of the 120 years from its occupation by the Spaniards to 1648, and in them find no indications whatsoever of yellow fever. Finally, in 1648, when yellow fever did occur, recognizable with certainty, it was in an epidemic of a sort which had been impossible in a population where the disease had been endemic within a generation or more. This epidemic is, indeed, strong evidence of the prior freedom from infection not only of Yucatan, but also of the Mexican littoral toward Vera Cruz.

Finlay, Bérenger-Féraud, and others have accepted as yellow fever the sickness and mortality which occurred at Hispaniola (Hayti) in the first years of its settlement, following the battle of Vega Real, in 1495. An examination of the contemporary accounts shows no sufficient evidence for this belief. Heavy as was the mortality, it is reasonably accounted for by famine and other hardships and by diseases other than yellow fever, especially malaria. Also, the subsequent history of Hispaniola, especially the absence of recorded epidemics aboard ship there until much later, is against the early existence of yellow fever. If the infection was not prevalent at Hispaniola, then the claim that Spaniards stopping there acquired immunity and so escaped the disease at Vera Cruz and elsewhere on the mainland is invalid—and this is a necessary part of the argument for American origin.

The losses suffered by the expeditions of Ojeda and Nicuesa to the Isthmus of Panama have been attributed to yellow fever. The history of these expeditions, examined in detail, shows no evidence for this, and much against it.

The "*modorra*" which occurred at Darien among the men who came with Pedrarias Davila in 1514, is, in the circumstances of its occurrence, more like yellow fever than any other epidemic described in the early records. However, there is nothing so to identify it; its duration was longer than would seem consistent with yellow fever; and other causes—famine and malaria especially—would seem sufficient to account for the sickness and mortality.

The "*coup de barre*" which prevailed at Guadeloupe soon after its settlement, in 1635, has been very generally accounted as yellow fever, and as having been long known to the inhabitants of the island. From the descriptions given it does not particularly resemble yellow fever, nor is there sufficient evidence that it was a disease previously known to the natives. That yellow fever, which visited Guadeloupe in 1648, was



then looked upon as a *new* disease and prevailed as an *epidemic*, is evidence against the "*coup de barre*" having been the same.

In Cuba, yellow fever was epidemic in 1648 or 1649, more probably the latter year, which implies that it was not endemic up to that time, and confirms other evidence of widespread epidemic prevalence in the Caribbean region about the time of the Yucatan epidemic.

Taking into view the whole of the Caribbean country—not any one place alone, but all of it together—considering the circumstances of the European invasion and settlement, and the kind of records that we have of it, the evidence is very strong—we think conclusive—that yellow fever was not endemic anywhere in this region when it was visited by Columbus, or for a long time thereafter. When it is identified here, in 1648, there had already been time enough for the introduction from Africa not only of the causative organism, but of its insect vector as well, and for the permanent implantation and wide dissemination of the latter.<sup>2</sup> Indeed, unless *aegypti* were lacking at first, one could not account for the long interval between the opening up of communications with the Old World and the first definitive appearance of the disease in America.

If the exclusion of America is certain, this, with what is known of the epidemiology of yellow fever, establishes West Africa as necessarily its place of origin. In support of this is other evidence, both historical and biological.

To say that historical evidence—except by exclusion of America—indicates Africa as the place of origin may be somewhat paradoxical, for, as we have said, the report of an epidemic that can be certainly identified as yellow fever comes to us 130 years earlier from America than a similar one does from Africa. However, conditions for the identification of yellow fever in early days were less favorable in Africa than in America. The reasons for this are: (1) The European histories of West Africa during the time of its discovery and settlement are far less complete than we have for America, and there are no native records whatsoever. (2) The European settlements, except on the islands, were small, and the expeditions into the country were made by relatively small bodies

<sup>2</sup> It is not intended to imply that 1648 is the date of the *first* occurrence of yellow fever anywhere in America. In all probability it had occurred before that in various places, its dissemination limited at first by the limited distribution of its vector. Repeated introductions into a place where the area of *aegypti* infestation was gradually extending might cause a series of *small* epidemics and eventual permanent establishment without any one sweeping epidemic such as occurred in Yucatan and Cuba.

of men, so that there are few instances of susceptible people introduced into the country in such numbers at any one time as would probably indicate to us the existence of yellow fever, when present. (3) From our earliest knowledge of West Africa, malaria has been very prevalent and very severe there, so that it is extremely difficult—usually impossible in the circumstances presented to us in early accounts—to distinguish between it and yellow fever. For these reasons, failure to find clear evidence of yellow fever in the early history of Africa has far less significance than a similar negative history in America.

Moreover, the early history of West Africa is not so thoroughly negative as that of America. It is true that not until 1778 do we find certain and direct record of yellow fever in Africa, yet the sickness in Drake's expedition of 1585, in that of Van der Does in 1599, and of Mascarhenas in 1638, although not certainly showing yellow fever, are indicative of it. Similarly, in the history of the settlement of São Thomé, the implications of the existence of a disease producing immunity on the Angola coast, as well as the descriptions of certain diseases occurring in São Thomé itself, are very suggestive of yellow fever. The same may be said, though it may be in lesser degree, of the high mortality in various commercial expeditions to Guinea. All of these occurred before—and some of them long before—1648, the date of the first certain yellow fever noted in America, and while they may not *certainly* indicate the presence of yellow fever in Africa at this time, yet, taken together, they do indicate it, and we have found nothing in America prior to 1648 which does so at all.

The biological evidence which indicates Africa as the place of origin of yellow fever is:

(1) The African negro contracts yellow fever, so far as we know, as readily as other races, but has it more mildly and rarely dies of it. His reaction to yellow fever is just what one would expect to have been evolved in a race for many generations subject to that infection. No other known race shows this reaction—certainly not the American Indian.

(2) Of the insect vector, *Aedes (Stegomyia) aegypti*, it has been said in an earlier chapter: "It would seem that this species, so definitely differentiated in its biology as a commensal of man, and not regarded by entomologists as one of the older forms, developed from some similar one with less sharply marked characteristics, and that, in the region where this took place, one would be apt to find a number of forms allied to it, but less completely differentiated; that is, different species of the



same subgenus and with similar, but not quite the same, life history." Now there are many species of *Stegomyia* in West Africa and elsewhere in the Old World, some of them of biology fairly similar to that of *aegypti*; but of such related species there are none listed in the Americas. That *Aedes* (*Stegomyia*) *aegypti* at present has a wide distribution in the New World, is, of course, no evidence for its having originated there, for wherever it existed originally, it would, with the establishment of maritime communication, be transported eventually to all ports, and would become implanted wherever the requisite conditions of climate and breeding places were found.

The biological evidence, as regards both the insect vector and the human host, is, so far as it goes, altogether in favor of an Old World—hence African—origin of yellow fever, and this, we find, is entirely consistent with the historical evidence. If, by itself, neither kind of evidence, biological or historical, is altogether conclusive, we think that together they make a very strong argument.

## BIBLIOGRAPHY\*

ANALES DE CHIMALPAHIN *See* Chimalpahin

ANALES DE CUAUHTITLAN

Anales de Cuauhtitlan; noticias historicas de Mexico y sus contornos, compiladas por D. José Fernando Ramirez y traducidas por los señores Faustino Galicia Chimalpapoca, Gumesindo Mendoza y Felipe Sanchez Solis. México, Imprenta de I. Escalante, 1885.

ANALES DE TECAMACHALCO

Anales de Tecamachalco; cronica local y colonial en idioma nahuatl. 1398 y 1590. [México, Oficina tip. de la Secretaría de fomento, 1903] (Colección de documentos para la historia mexicana, pub. por el Dr. A. Peñafiel. 5. cuaderno)

ANSTIE, FRANCIS EDMUND

1866. Notes on epidemics. For the use of the public . . . 1st American ed. Philadelphia, J. B. Lippincott & co.

AREJULA, JUAN MANUEL DE

1806. Breve descripción de la fiebre amarilla padecida en Cadiz y pueblos comarcanos en 1800, en Medina-Sidonia en 1801, en Malaga en 1803 y en esta misma plaza, y varias otras del reyno en 1804. Madrid, Imprenta real.

ARROTT, JAMES

1844. Letter on the present epidemic of Dundee. *Scottish and North of England med. gaz.*, 1: 129-133.

ASENSIO Y TOLEDO, JOSÉ MARÍA

1898-1900. Relaciones de Yucatán. Madrid, Est. tip. "Sucesores de Rivadeneyra," 2 v. (Colección de documentos inéditos relativos al descubrimiento, conquista y organización de las antiguas posesiones españoles de ultramar. 2 ser., t. 11, 13)

ASSELINE, DAVID, *d.* 1703.

1874. Les antiquitez et chroniques de la ville de Dieppe . . . Dieppe, A. Marais; Paris, Maisonneuve et c<sup>e</sup>, 2 v. (Bibliothèque dieppoise)

AUDOUARD, MATHIEU FRANÇOIS MAXENCE

1818. Recherches sur la contagion des fièvres intermittentes. Paris, Méquignon-Marvis.

1822. Relation historique et médicale de la fièvre jaune qui a régné à Barcelone en 1821. Paris, Moreau, imprimeur.

1824. Considérations hygiéniques sur le typhus nautique ou fièvre jaune. [Paris] Gueffier. Extr. *Rev. médicale et jour. de clin.*, nov., 1824.

1826. Aperçus généraux sur l'infection et la contagion dans la peste et la fièvre jaune. [Paris] Impr. de Gueffier. Extr. *Rev. médicale et jour. de clin.*, avril, 1826.

---

\* Dates prefixed to titles are imprint dates.



AUGUSTIN, GEORGE

1909. History of yellow fever . . . New Orleans, Searcy & Pfaff, ltd.

AZURARA. See Eannes de Azurara, Gomes

BACOT, A.

1918. A note on the period during which the eggs of *Stegomyia fasciata* (*Aedes calopus*) from Sierra Leone stock retain their vitality in a humid temperature. *Parasitology*, Cambridge, 10: 280-283.

BAERLE, KASPAR VAN

1660. Casparis Barlæi Rervm per octennivm in Brasilia et alibi gestarum . . . Clivis, ex officina Tobiaë Silberling.

BALFOUR, Sir ANDREW

1920-1921. A medical and sanitary survey of Mauritius: past, present and future. *Trans. Royal soc. trop. med. & hyg.*, 15: 157-176.

BALLOT, VICTOR ALEXIS LOUIS FRANÇOIS

1870. Note à l'appui de la théorie du développement spontané de la fièvre jaune épidémique dans les Petites Antilles. *Arch. de méd. navale*, 13: 54-62.

BANCROFT, EDWARD NATHANIEL

1811. An essay on the disease called yellow fever . . . London, Printed for T. Cadell and W. Davies.

BARBOT, JEAN

1732. A description of the coasts of north and south-Guinea, and of Ethiopia inferior, vulgarly Angola. (*In Churchill, Awnsham, comp. A collection of voyages and travels. London, v. 5*)

BARROS, JOÃO DE, 1496-1570.

1628. Decada primeira da Asia . . . Lisboa, I. Rodriguez.

BEAZLEY, CHARLES RAYMOND

1899. Introduction on the early history of African exploration, cartography, etc. (*In Eannes de Azurara, Gomes. Chronicle of the discovery and conquest of Guinea . . . London, Printed for the Hakluyt society, vol. ii, pp. i-cl*)

BENZONI, GIROLAMO

1565. La historia del mondo nvovo . . . [1st ed.] Venetia, F. Rampazetto.

BÉRENGER-FÉRAUD, LAURENT JEAN BAPTISTE

1878. De la fièvre dite bilieuse inflammatoire aux Antilles et dans l'Amérique tropicale . . . Paris, A. Delahaye & cie.

1890. Traité théorique & clinique de la fièvre jaune. Paris, O. Doin.

BEYER, G. E., POTHIER, O. L., COURCET, M., AND LEHMANN, I. I.

1902. Bionomics, experimental investigations with *Bacillus sanarelli* and experimental investigations with malaria in connection with the mosquitoes of New Orleans. *New Orleans med. & surg. jour.*, 14: 419-480.

BIGGES, WALTER, d. 1585?

1652. A summarie and true discourse of Sir Francis Drakes West-Indian voyage . . . Printed at London, for Nicholas Bourne, 1652. (*In Sir Francis Drake revived . . . collected out of the notes of the said Sir Francis Drake . . . and . . . divers other gentlemen. London, 1653*)

BIONDELLI, BERNARDINO

1869. Glossarium azteco-latinum et latino-aztecum. Mediolani, apud Valentiner et Mues.

BOOK OF THE JEW. *See* Libro del Judio.

BOR, PIETER CHRISTIAANSZON

- 1621-1634. Nederlantsche oorloghen, beroerten, ende borgerlijke oneenicheyden . . . Leyden, G. Bapon; Amsterdam, M. Colijn, 5 v. in 6.

BOUTON, JACQUES

1640. Relation de l'establissement des Francois depvis l'an 1635. En l'isle de la Martinique, l'vne des Antilles de l'Amerique . . . Paris, S. Cramoisy.

BOYCE, Sir RUBERT WILLIAM

1910. The prevalence, distribution and significance of *Stegomyia fasciata*, F. (= *calopus*, Mg.) in West Africa. *Bull. entomol. research*, 1: 233-263.  
1911. Yellow fever and its prevention. London, J. Murray.

BRETON, RAYMOND

1665. Dictionnaire caraibe-françois . . . Avxerre, G. Bovqvet.  
1666. Dictionnaire françois-caraibe . . . Avxerre, G. Bovqvet.

BRINTON, DANIEL GARRISON

1881. The names of the gods in the Kiche myths, Central America. Philadelphia, McCalla & Stavely, prs.  
1882. The Maya chronicles. Philadelphia, D. G. Brinton. (Library of aboriginal American literature, no. 1)  
1891. The American race: a linguistic classification and ethnographic description of the native tribes of North and South America . . . New York, N. D. C. Hodges.  
1894. Nagualism. A study in native American folk-lore and history . . . Philadelphia, MacCalla & company, prs.

BRITO FREIRE, FRANCISCO DE

1675. Nova Lusitania, historia da guerra brasilica . . . Lisboa, J. Gabram.

BRUCE, ALEXANDER

1759. An inquiry concerning the cause of the pestilence, and the diseases in fleets and armies . . . Edinburgh, S. Bladon.

BRUMPT, ÉMILE

1922. Précis de parasitologie. 3<sup>e</sup> éd. Paris, Masson et c<sup>ie</sup>.

BUCHANAN, GEORGE

1866. Report on the outbreak of yellow fever at Swansea. (*In* Eighth report of the medical officer of the Privy council. 1865. London, Printed by G. E. Eyre and W. Spottiswoode, p. 440-468)

BUNBURY, Sir EDWARD HERBERT, *bart.*

1883. History of ancient geography. London, J. Murray, 2 v.

BURNETT, Sir WILLIAM

1814. A practical account of the fever commonly called the bilious remittent, as it appeared in the ships and hospitals of the Mediterranean fleet . . . London, Printed for J. Callow.

CANARY ISLANDS. *See* Great Britain, *Foreign Office. Historical Section.*

CAPE VERDE ISLANDS. *See* Great Britain, *Foreign Office. Historical Section.*



## CARTER, HENRY ROSE

- 1900-a. A note on the interval between infecting and secondary cases of yellow fever, from the records of the yellow fever at Orwood and Taylor, Miss., in 1898. *New Orleans med. & surg. jour.*, 52: 617-636.  
Also reprinted in Ann. rept. of the supervising surg. genl. of the U. S. Marine-hosp. service for the year 1900, pp. 230-241. Washington, Govt. print. off., 1901.
- 1900-b. Yellow fever mortality in Havana, 1884-1900: statistics and deductions. *Public health repts.*, U. S. Marine-hosp. service, 15: 1840-1853.
- 1901-a. The period of incubation of yellow fever: a study from unpublished observations. *Med. record*, 59: 361-367.
- 1901-b. A note on the spread of yellow fever in houses. Extrinsic incubation. *Med. record*, 59: 933-937.  
Also in Ann. rept. of the supervising surg. genl. of the U. S. Marine-hosp. service, for the year 1900, pp. 242-252. Washington, Govt. print. off., 1901.
- 1901-c. A correlation of some facts in the propagation of yellow fever with the theory of its conveyance by the *Culex fasciatus*. *Philadelphia med. jour.*, 7: 694-696.
1902. Are vessels infected with yellow fever? Some personal observations. *Med. record*, 61: 441-444.
- 1904-a. Some characteristics of *Stegomyia fasciata* which affect its conveyance of yellow fever. *Med. record*, 65: 761-766.
- 1904-b. The conveyance of yellow fever. *Med. news*, 85: 878-884.
1906. A memorandum from Panama experience, showing that yellow fever was contracted in the daytime. *Med. record*, 69: 683.
1909. Notes on the sanitation of yellow fever and malaria. From Isthmian experience. *Med. record*, 76: 56-59.
1914. Yellow fever, its epidemiology, prevention and control. *Pub. health repts.*, U. S. Pub. health service, suppl. no. 19. Washington, Govt. print. off.
1915. Immunity in yellow fever. *Trans. Soc. trop. med. & hyg.*, 8: 279-282.
1916. Immunity to yellow fever. *Ann. trop. med. & parasitol.*, 10: 153-164.
1917. Spontaneous disappearance of yellow fever from failure of the human host. *Trans. Soc. trop. med. & hyg.*, 10: 119-129.
1920. The mechanism of the spontaneous elimination of yellow fever from endemic centers. *Ann. trop. med. & parasitol.*, 13: 299-311.
1922. Yellow fever in Peru: epidemic of 1919 and 1920. *Am. jour. trop. med.*, 2: 87-106.
- 1924-a. Preferential and compulsory breeding places of *Aedes (Stegomyia) aegypti* and their limits. *Ann. trop. med. & parasitol.*, 18: 493-503.
- 1924-b. The chance of the extension of yellow fever to Asia and Australasia. (In Proceedings of the Pan-Pacific science congress, Australia, 1923. Melbourne, H. J. Green, govt. printer, 1924, 2: 1377-1388)
- CASAS, BARTOLOMÉ DE LAS, 1474-1566.  
1875-76. Historia de las Indias . . . Madrid, Impr. de M. Ginesta, 5 v.
- CAVASSA, NICOLAS E.  
1922. Algunas observaciones sobre el estegomyia calopus. *La Crónica médica*, Lima, 39: 484-488.

## CHAILLÉ, STANFORD EMERSON

1882. Report to the United States national board of health on yellow fever in Havana and Cuba. (*In Annual report of the National board of health, 1880, p. 69-308. Washington, Govt. print. off.*)

## CHAUVERO, ALFREDO

- 1867-80. Explicación del código geroglífico de Mr. Aubin. (*In Durán, Diego. Historia de las Indias de Nueva-España . . . México, 1867-80, t. 2 (1880) 172 p. at end*) Description and facsimiles of liturgical calendar now known as the "Codex Ixtlilxochitl."

## CHILAM BALAM OF CHUMAYEL, BOOK OF

- Book of Chilam Balam of Chumayel; Maya ms. 107 p., sm. 4°. (Original lost; copy by Berendt in Berendt linguistic collection, no. 49; reproduced by George Byron Gordon *in Anthropological publications of the University of Pennsylvania, v. 5. Philadelphia, 1913*)

## CHILAM BALAM OF TIZIMIN, BOOK OF

- Book of Chilam Balam of Tizimin (Codice anónimo) Maya ms. 52 p., 4° (Original lost; copy by Berendt (1868) in Berendt linguistic collection, University of Pennsylvania, no. 49) Another copy in Peabody museum (Cambridge, Mass.) Gates collection.

## CHIMALPAHIN QUAUHTLEHUANITZIN, DOMINGO FRANCISCO DE SAN ANTON MUÑOZ, b. 1579.

- Annales de Domingo Francisco de San Anton Muñoz Chimalpahin Quauhtlehuanitzin, sixième et septième relations (1258-1612) pub. et tr. sur le manuscrit original par Rémi Siméon . . . Paris, Maisonneuve et C. Leclerc, 1889. (Bibliothèque linguistique américaine, t. xii)

## CHIRAC, PIERRE

1742. Traité des fièvres malignes; des fièvres pestilentiellles, et autres . . . Paris, J. Vincent, 2 v.

## CHISHOLM, COLIN

1801. An essay on the malignant pestilential fever, introduced into the West Indian Islands from Boullam, on the coast of Guinea, as it appeared in 1793, 1794, 1795 and 1796 . . . 2d ed. London, Printed for J. Mawman, 2 v.

## CHRISTOPHERS, S. R., AND SHORTT, H. E.

1921. Malaria in Mesopotamia. *Indian jour. med. research*, 8: 508-552.

## CHURCHILL, AWNSHAM, AND CHURCHILL, JOHN

1732. A collection of voyages and travels . . . London, J. Walthoe, 6 v.

## CLARKE, JAMES STANIER

1803. The progress of maritime discovery, from the earliest period to the close of the eighteenth century. [vol. i] London, T. Cadell, and W. Davies.

## CODEX AUBIN

- Código Aubin; manuscrito azteca de la Biblioteca real de Berlin, anales en mexicano y geroglíficos desde la salida de las tribus de Aztlan hasta la muerte de Cuauhtemoc. Mexico, Oficina tip. de la Secretaría de fomento, 1902. (Colección de documentos para la historia mexicana . . . 4. cuaderno)

## CODEX IXTLILXOCHITL. See Chauvero, Alfredo



## CODEX RIOS

Codex Rios; copy of a Mexican manuscript preserved in the Library of the Vatican . . . . marked no. 3738. (*In* Kingsborough, Edward King, *viscount*, *Antiquities of Mexico* . . . . London, 1830, v. 2, pp. 1-149)

## CODEX TELLERIANO-REMENSIS

Codex Telleriano-Remensis . . . . précédé d'une introduction . . . . par le d<sup>r</sup>. E. T. Hamy . . . . Paris, 1899.

COGOLLUDO. *See* López de Cogolludo, Diego

## CONNOR, MICHAEL EDWARD

1923. Notes on yellow fever in Mexico. *Am. jour. trop. med.*, 3: 105-116.

## CONNOR, M. E., AND MONROE, W. M.

1923. *Stegomyia* indices and their value in yellow fever control. *Am. jour. trop. med.*, 3: 9-19.

## CORDEIRO, LUCIANO

1892. Descobertas e descobridores: Diogo D'Azambuja. Lisboa, Impr. nacional.

## CORMACK, Sir JOHN ROSE

1843. Natural history, pathology, and treatment of the epidemic fever, at present prevailing in Edinburgh and other towns . . . . London, J. Churchill.

## CORNILLIAC, JEAN JACQUES

1873. Études sur la fièvre jaune à la Martinique de 1669 à nos jours. 2. éd. Fort-de-France, Imp. du gouvernement.

1886. Recherches chronologiques et historiques sur l'origine et la propagation de la fièvre jaune dans les Antilles et sur la côte occidentale d'Afrique. Fort-de-France, Impr. du gouvernement.

## CORRE, ARMAND MARIE

1882. De l'étiologie et de la prophylaxie de la fièvre jaune. Paris, O. Doin.

## CORTÉS, HERNANDO, 1485-1547.

1866. Cartas y relaciones de Hernan Cortés al emperador Carlos V; colegidas é ilustradas por Don Pascual de Gayangos . . . Paris, A. Chaix y ca.

1908. Letters of Cortes: the five letters of relation from Fernando Cortes to the Emperor Charles V. Tr. and ed. by Francis Augustus MacNutt . . . . New York and London, G. P. Putnam's sons.

## CRAGG, FRANCIS WILLIAM

1921-22. The epidemiology of relapsing fever in India. *Trans. Royal soc. trop. med. & hyg.*, 15: 236-252.

## CRAIGIE, DAVID

1843. Notice of a febrile disorder which has prevailed at Edinburgh during the summer of 1843. *Edinburgh med. & surg. jour.*, 60: 410-418.

## DAMPIER, WILLIAM

1698-1703. A new voyage round the world . . . . London, Printed for J. Knapton, 3 v.

## DAPPER, OLFERT

1676. Naukeurige beschrijvinge der Afrikaensche eylanden . . . . Amsterdam, J. van Meurs.

DEL MONTE. *See* Monte y Tejada, Antonio del

DESGENETTES, RÉNÉ NICOLAS DUFRICHE, *baron*

1802. Histoire médicale de l'armée d'Orient . . . . Paris, Croullebois.

DÍAZ DEL CASTILLO, BERNAL, 1492-1581?

Historia verdadera de la conquista de la Nueva España . . . . México, Oficina tipográfica de la Secretaría de fomento, 1904-05, 2 v.

Histoire véridique de la conquête de la Nouvelle-Espagne . . . . Traduction par D. Jourdanet. 2. éd. . . . Paris, G. Masson, 1877.

DRAKE, DANIEL

1850. A systematic treatise, historical, etiological and practical, on the principal diseases of the interior valley of North America . . . . Cincinnati, W. B. Smith & co.; New York, Mason & Law.

DUNN, LAWRENCE H.

1923. Prevalence of the yellow fever mosquito, *Aedes calopus*, in the southern part of Peru. *Am. jour. trop. med.*, 3: 1-8.

DURÁN, DIEGO, *d.* 1588?

Historia de las Indias de Nueva-España y islas de Tierra Firme. México, Impr. de J. M. Andrade y F. Escalante, 1867-80, 2 v. and atlas.

DU TERTRE, JEAN BAPTISTE

1667-71. Histoire generale des Antilles habitées par les François. Paris, T. Iolly, 4 v.

DUTROULAU, AUGUSTE FRÉDÉRIC

1868. Traité des maladies des Européens dans les pays chauds (régions tropicales) 2. éd. Paris, J. B. Baillière et fils.

DYAR, HARRISON GRAY

1920. The earliest name of the yellow fever mosquito. *Insecutor inscitiae menstruus*, Washington, 8: 204.

EANNES DE AZURARA, GOMES, *15th cent.*

Chronica do descobrimento e conquista de Guiné . . . . trasladada . . . . por diligencia do Visconde da Carreira . . . . Pariz, J. P. Aillaud, 1841. The chronicle of the discovery and conquest of Guinea . . . . now first done into English by Charles Raymond Beazley . . . . and Edgar Prestage . . . . London, Printed for the Hakluyt society, 1896-99, 2 v. (Works issued by the Hakluyt society, no. xcv, c)

EDEN, RICHARD

1577. The History of Trauayle in the VWest and East Indies. London, Imprinted by Richarde Iugge.

ELLIS, ALFRED BURDON

1893. A history of the Gold Coast of West Africa. London, Chapman and Hall, ltd.

ESPINOSA, ALONSO DE, *16th cent.*

The Guanches of Tenerife, the holy image of Our Lady of Candelaria, and the Spanish conquest and settlement . . . . Tr. and ed . . . . by Sir Clements Markham. London, Printed for the Hakluyt society, 1907. (Works issued by the Hakluyt society . . . . 2d ser., no. xxi)

ESTANCELIN, LOUIS, 1777-1858.

Dissertation sur les découvertes faites par les navigateurs dieppois. Abbeville, Impr. de Boulanger-Vion, 18-



## FAGET, JEAN CHARLES

1875. The type and specificity of yellow fever established with the aid of the watch and thermometer. Paris, J. B. Baillière & sons.

## FARIA E SOUSA, MANUEL DE, 1590-1649.

1730. Historia del reyno de Portugal . . . . Amberes, Juan Bautista Verdussen.

## FERGUSON, WILLIAM

1839. Observations on yellow fever. *London med. gaz.*, n.s., 24: 838-843.

## FERGUSON, WILLIAM, 1773-1846.

1823. On the nature and history of the marsh poison. *Trans. Royal. soc. of Edinburgh*, 9: 273-298.

## FERREIRA DA ROSA, JOÃO

1694. Trattado unico da constituicão pestilencial de Pernambuco. Lisboa, M. Manescal.

## FINLAY, CARLOS JUAN

1904. Yellow fever: historical sketch of the disease, its etiology and mode of propagation. (*In A reference handbook of the medical sciences . . . .* ed. by A. H. Buck, vol. viii, pp. 322-332. New York, W. Wood and co.)
1912. Trabajos selectos. Habana, Secret. de san. y benefic. (Contains numerous articles previously published in various periodicals, 1881-1907)

## FISKE, JOHN

1899. The beginnings of New England . . . . Boston and New York, Houghton, Mifflin and company.
1900. Old Virginia and her neighbors . . . . Cambridge, Riverside press, 2 v.

## FLU, PAUL CHRISTIAN

1910. Beobachtungen während der gelbfieberepidemie, die von dezember 1908 bis februar 1909 in Paramaribo herrschte. *Zeitschr. f. hyg. u. infektionskrank.*, 65: 17-54.
1920. Tests re the period in which mosquitoes can live at low temperatures. *Mededeel. v.d. burgerl. geneesk. dienst. in Nederl. Indie. Batavia*, 7: 99-105.

## FOWLER, C. E. P.

1908. Malarial investigations in Mauritius from November, 1907, to February, 1908. Report. London, Printed for H. M. Stationery off.

## FRANCIS, EDWARD

1907. Observations on the life cycle of *Stegomyia calopus*. *Pub. health repts.*, U. S. Pub. health and mar.-hosp. service, 22 (pt. I): 381-383.

## GAFFAREL, PAUL LOUIS JACQUES

1880. Les colonies françaises. Paris, G. Baillière et c<sup>ie</sup>.

## GARCÍA ICAZBALCETA, JOAQUÍN

1881. Don fray Juan de Zumárraga, primer obispo y arzobispo de México; estudio biográfico y bibliográfico . . . . México, Andrade y Morales.
- 1886-92. Nueva colección de documentos para la historia de México. México, Andrade y Morales, sucesores, 5 v. in 3.

## GÖLDI, EMIL AUGUST

1905. . . . Os mosquitos no Pará . . . . Reuniões de quatro trabalhos sobre os mosquitos indigenas, principalmente as especies que molestam o homen . . . . Pará, Brazil, C. Wiegandt. (Memorias do Museu Gœldi (Museu paraense) de historia natural e ethnographia, iv)

GORDON, R. M., AND YOUNG, C. J.

1921. The feeding habits of *Stegomyia calopus*, Meigen. *Ann. trop. med. & parasitol.*, 15: 265-268.

GORGAS, WILLIAM CRAWFORD

1908. Method of the spread of yellow fever. *Med. record*, 73: 1061-1063.  
1918. Sanitation in Panama. New York, D. Appleton and co.

GRAVES, ROBERT JAMES

1848. Clinical lectures on the practice of medicine. 2d ed. Dublin, Fannin and co., 2 v.

GRAVES, R. J., AND STOKES, W.

1827. Clinical reports of the medical cases in the Meath hospital and county of Dublin infirmary, during the session 1826, 1827. part I. Dublin.

GREAT BRITAIN, FOREIGN OFFICE. HISTORICAL SECTION

- 1920-a. . . . . Canary Islands. London, H. M. Stationery off. (Handbooks . . . . . no. 123)  
1920-b. . . . . Cape Verde Islands. London, H. M. Stationery off. (Handbooks . . . . . no. 117)  
1920-c. . . . . San Thomé and Principe. London, H. M. Stationery off. (Handbooks . . . . . no. 119)

GREAT BRITAIN, GENERAL BOARD OF HEALTH

1852. Second report on quarantine. Yellow fever . . . . London, Printed by W. Clowes & sons, for H. M. Stationery off.

GUITERAS, JUAN

1888. Some observations on the natural history of epidemics of yellow fever . . . . (In Ann. rept. of the supervising surg. genl. of the U. S. Marine-hosp. service, for the fiscal year 1888, pp. 75-105. Washington, Govt. print. off.)  
1917. Ictero epidemico observado en Barbadas (Antillas inglesas) *Sanidad y beneficencia, boletin oficial de la Secretaría*, 18: 241-256.  
1921-a. Expedition al Africa y estudios de fiebre amarilla. *Sanidad y beneficencia, boletin oficial*, 25: 21-33.  
1921-b. Observations on yellow fever in a recent visit to Africa. *Arch. of diagnosis*, 14: 1-14.

GUYON, JEAN LOUIS GENEVIÈVE

1858. Un mot sur la fièvre jaune de Lisbonne en 1857. *Gazette méd. de Paris*, 13: 449-454.

HAKLUYT, RICHARD, 1552?-1616.

- 1885-90. The principal navigations, voyages, traffiques, and discoveries of the English nation . . . . Edinburgh, E. & G. Goldsmid, 16 v.

HAWKINS, Sir RICHARD

1622. The observations of Sir Richard Hawkins knight, in his voiage into the South Sea. Anno Domini 1593 . . . . London, Printed by I. D. for I. Iaggard.

HAWORTH, W. E.

1924. Mosquitoes and cocoanut palms. A mosquito survey of palm trees in East Africa and the problems resulting therefrom. *Trans. Royal soc. trop. med. & hyg.*, 18: 162-196.



HERRERA Y TORDESILLAS, ANTONIO DE, 1559-1625.

- 1726-27. *Historia general de los hechos de los castellanos en las islas i tierra firme del mar oceano . . . .* En Madrid en la Imprenta real de Nicolas Rodriguez [!] Franco, 9 v. in 5.

HILLARY, WILLIAM

1759. *Observations on the changes of the air and the concomitant epidemical diseases, in the island of Barbados. To which is added A treatise on the putrid bilious fever, commonly called the yellow fever; and such other diseases as are indigenous or endemial, in the West India Islands, or in the torrid zone.* London, Printed for C. Hitch and L. Hawes.

HIRSCH, AUGUST

1883. *Handbook of historical and geographical pathology.* Tr. from the 2d German ed. by Charles Creighton. London, New Sydenham society, 3 v.

HOWARD, LELAND OSSIAN

1905. . . . Concerning the geographic distribution of the yellow fever mosquito. *Pub. health reports.* U. S. Marine-hosp. service, Suppl. vol. xviii, no. 46, Nov. 13, 1903. Rev. to Sept. 10, 1905. Reprint no. 16.

HOWARD, L. O., DYAR, H. G., AND KNAB, F.

- 1912-17. *The mosquitoes of North and Central America and the West Indies.* Washington, Carnegie institution of Washington, 4 v. in 3. (Carnegie institution of Washington. Publication no. 159)

HUGHES, GRIFFITH

1750. *The natural history of Barbados . . . .* London, Printed for the author.

HUMBOLDT, ALEXANDER, VON

1811. *Essai politique sur le royaume de la Nouvelle-Espagne . . . .* Paris, F. Schoell, 5 v.

ICAZBALCETA. *See* García Icazbalceta, Joaquín

IRVING, WASHINGTON, 1783-1859.

1892. *The life and voyages of Christopher Columbus and the Voyages and discoveries of the companions of Columbus.* Isabella edition. New York, G. P. Putnam's sons, 2 v.

JESUITS. *LETTERS FROM MISSIONS (SOUTH AMERICA)*

*Mission de Cayenne et de la Guyane française.* Paris, Julien, Lanier, Cosnard et c<sup>e</sup>, 1857. (*Voyages et travaux des missionnaires de la Compagnie de Jésus . . . . I*)

JOHNSON, JAMES

1826. *The influence of tropical climates on European constitutions: being a treatise on the principal diseases incidental to Europeans in the East and West Indies, Mediterranean, and coast of Africa . . . .* From the 3d London ed. New York, E. Duyckinck, G. Long.

JONES, JAMES

1858. *Outlines of lectures on yellow fever.* *New Orleans med. & surg. jour.*, 15: 500-517, 697-710.

## LABAT, JEAN BAPTISTE

1722. Nouveau voyage aux isles de l'Amerique . . . Paris, P. F. Giffart, 6 v.  
 1728. Nouvelle relation de l'Afrique occidentale . . . Paris, G. Cavelier, 4 v. in 2.  
 1731. Voyage du chevalier Des Marchais en Guinée, isles voisines, et à Cayenne . . . Amsterdam, Aux dépens de la Compagnie, 4 v.

## LAET, JOANNES DE

1644. Historie ofte Iaerlijck verhael van de verrichtinghen der Geoctroyeerde West-Indische compagnie . . . Leyden, Bonaventuer ende Abraham Elsevier.

## LANDA, DIEGO DE, 1524-1579.

1864. Relation des choses de Yucatan . . . Paris, A. Durand. (Collection de documents dans les langues indigènes, pour servir à l'étude de l'histoire et de la philologie de l'Amérique ancienne, v. 3)

## LA ROCHE, RENÉ

1855. Yellow fever, considered in its historical, pathological, etiological, and therapeutical relations . . . Philadelphia, Blanchard and Lea, 2 v.

LARREY, DOMINIQUE JEAN, *baron*

1803. Relation historique et chirurgicale de l'expédition de l'armée d'Orient en Égypte et en Syrie. Paris, Demonville & sœurs.

- 1812-17. Mémoires de chirurgie militaire, et campagnes. Paris, J. Smith, 4 v.

LAS CASAS. *See* Casas, Bartolomé de las

## LEBLOND, JEAN BAPTISTE

1805. Observations sur la fièvre jaune, et sur les maladies des tropiques . . . Paris, T. Barrois.

## LE PRINCE, J. A., AND ORENSTEIN, A. J.

1916. Mosquito control in Panama. New York, G. P. Putnam's sons.

## LE ROY DE MÉRICOURT, ALFRED

1867. Iles Canaries. *Arch. de méd. navale*, Paris, 7: 241-255.

## LIBRO DEL JUDIO

Libro del Judio; ms. en lengua Maya. 156 p. (Original in Peabody museum (Cambridge, Mass.) Gates reproduction)

## LIGON, RICHARD

1673. A true & exact history of the island of Barbadoes . . . [2d ed.] London, P. Parker and T. Guy.

## LIND, JAMES

1757. A treatise on the scurvy . . . 2d ed. London, O. Millar.  
 1792. An essay on diseases incidental to Europeans in hot climates . . . 5th ed. London, Printed for J. Murray.

## LÓPEZ DE COGOLLUDO, DIEGO

1688. Historia de Yucathan . . . Madrid, J. Garcia Infanzon.

LOPES, FERNÃO, *b. ca.* 1380.

Chronica de el-rei D. João I. Lisboa, Escriptorio, 147 rua dos Retrozeiros, 1897-1900. 10 v. (Bibliotheca de classicos portuguezes, vii-viii)

## LOPEZ, FERNANDO

1905. Experimental study on the acclimation of the *Stegomyia fasciata* mosquito in Mexico City. Am. pub. health assn., *Pub. health papers and reports*, 30: 222-225.



## MACFIE, JOHN WILLIAM SCOTT

1915. Observations on the bionomics of *Stegomyia fasciata*. *Bull. entomol. research*, 6: 205-229.
1920. Heat and *Stegomyia fasciata*: short exposures to raised temperatures. *Ann. trop. med. & parasitol.*, 14: 73-82.

## MACGREGOR, MALCOLM EVAN

1915. Notes on the rearing of *Stegomyia fasciata* in London. *Jour. trop. med. & hyg.*, 18: 193-196.
1919. On the occurrence of *Stegomyia fasciata* in a hole in a beech tree in Epping Forest. *Bull. entomol. research*, 10 (pt. I): 91.

## M'KINLAY, WILLIAM

1852. Remarks on the yellow fever which appeared of late years on the coast of Brazil. *Monthly jour. med. science*, 15: 254-274, 335-352, 424-441.

## MAGENDIE, FRANÇOIS

1842. *Phénomènes physiques de la vie* . . . . Paris, J. B. Baillière, 4 v.

## MARCHOUX, É., SALIMBENI, A., AND SIMOND, P. L.

1903. La fièvre jaune; rapport de la mission française. *Ann. de l'Inst. Pasteur*, 17: 665-731.

## MARCHOUX, É., AND SIMOND, P. L.

1906. Études sur la fièvre jaune: deuxième rapport de la mission française. *Ann. de l'Inst. Pasteur*, 20: 16-40, 104-148, 161-205.

## MARTINEZ HERNANDEZ, JUAN

1926. Paralelismo entre los calendarios maya y azteca; su correlación con el calendario juliano . . . Mérida, Yuc., México, Compañía tipográfica yucateca, s.a. Repr.: *Diario de Yucatan*, Feb. 7, 1926.

## MASDEVALL, JOSEPH

1786. Relacion de las epidemias de calenturas pútridas y malignas, que en estos últimos años se han padecido en el principado de Cataluña . . . 2. ed. Madrid, Imprenta real.

## MÉLIER, FRANÇOIS

1863. Relation de la fièvre jaune survenue à Saint-Nazaire en 1861 . . . . Paris, J. B. Baillière et fils.
- Also in: *Mém. de l'Acad. de méd. de Paris*, 1863, 26: 1-228.

## MENDIETA, GERÓNIMO DE, d. 1604.

Historia eclesiástica indiana, obra escrita á fines del siglo xvi . . . la publica por primera vez Joaquín García Icazbalceta. México, Antigua librería, 1870.

## MERRIAM, CLINTON HART

1895. Geographic distribution of animals and plants in North America. (*In* U. S. Agriculture dept. Yearbook, 1894, pp. 203-214. Washington, Govt. print. off.)

## METEREN, EMANUEL VAN

1614. Belgica. Emanvels van Meteren Historie der Nederlandscher ende haerder naburen oorlogen ende geschiedenissen, tot den iare M. vi<sup>c</sup>. xii . . . . s'Graven-haghe, H. Iacobssz.

## MOLINA, ALONSO DE

1571. Vocabvlario en lengva Castellana y Mexicana . . . Mexico, Antonio de Spínosa, 2 v. in 1.

## MOLINA SOLIS, JUAN FRANCISCO

1896. Historia del descubrimiento y conquista de Yucatán . . . . Mérida de Yucatán, Imprenta R. Caballero.

1904-10. Historia de Yucatan durante la dominacion española . . . . t. 1-2. Mérida de Yucatan, Impr. de la Loteria del estado, 2 v.

## MONCHY, SOLOMON DE

1762. An essay on the causes and cure of the usual diseases in voyages to the West-Indies: together with the preservatives against them. London, Printed for T. Becket, and P. A. de Hondt.

## MONTE Y TEJADA, ANTONIO DEL

1853. Historia de Santo Domingo . . . . Tomo I. Habana, Soler.

## MOREAU DE JONNÈS, ALEXANDRE

1820. Monographie historique et médicale de la fièvre jaune des Antilles. Paris, Migneret.

1822. Notice sur la nature et l'origine de la maladie pestilentielle, dont les ravages furent éprouvés en 1816, par l'expédition de découvertes chargée de reconnaître le cours du Zaire. *Nouveau jour. de méd. chir. pharm. etc.*, 14: 330-346.

## MORLEY, SYLVANUS GRISWOLD

1915. An introduction to the study of the Maya hieroglyphs. Washington, Govt. print. off. (Smithsonian institution, Bureau of American ethnology, bull. 57)

1920. The inscriptions at Copán. Washington, The Carnegie institution of Washington.

## MOTLEY, JOHN LOTHROP

1861-68. History of the United Netherlands. New York, Harper and bros., 4 v.

MOTOLINIA, TORIBIO, *d.* 1568.

Historia de los indios de la Nueva España . . . . sácalos nuevamente a luz el R. P. Fr. Daniel Sánchez García . . . . teniendo a la vista las ediciones de Lord Kingborough [!] y de García Icazbalceta . . . . Barcelona, Herederos de J. Gili, 1914.

## NAVARRETE, MARTÍN FERNÁNDEZ DE

1825-37. Colección de los viages y descubrimientos que hicieron por mar los Españoles desde fines del siglo xv, con varios documentos inéditos concernientes á la historia de la marina castellana y de los establecimientos españoles en Indias. Madrid, Imprenta real, 5 v.

## NETSCHER, PIETER MARINUS

1853. Les Hollandais au Brésil . . . . La Haye, Belinfante frères.

## NOC, F., AND NOGUE, M.

1921. Ictères épidémiques et spirochètes au Sénégal. *Bull. Soc. path. exot.*, 14: 460-470.

## NOTT, JOSIAH CLARK

1847. Yellow fever contrasted with bilious fever, reasons for believing it a disease *sui generis*, its mode of propagation, remote cause, probable insect or animalcular origin . . . . *New Orleans med. & surg. jour.*, 4: 563-601.



NÚÑEZ DE LA PEÑA, JUAN

1676. Conqvista, y antigvedades de las islas de la Gran Canaria, y sv descripcion . . . . Madrid, Imprenta real, a costa de F. Anisson.

OVIEDO Y VALDÉS, GONZALO FERNÁNDEZ DE, 1478-1557.

- Historia general y natural de las Indias, islas y tierra-firme del mar océano . . . . Madrid, Impr. de la Real academia de la historia, 1851-55, 3 pts. in 4 v.

PELLEPRAT, PIERRE

1655. Relation des missions des pp. de la Compagnie de Iesvs dans les isles, & dans la Terre Ferme de l'Amerique meridionale . . . . Paris, S. & G. Cramoisy.

PÉREZ, JUAN Pío, 1798-1859.

- 1866-77. Diccionario de la lengua maya . . . . Mérida de Yucatan, J. F. Mólina Solis.

PERYASSÚ, ANTONIO GONÇALVES

1908. Os culicideos do Brazil. Inst. de Manguinhos, Rio de Janeiro.

PEZUELA Y LOBO, JACOBO DE LA

- 1863-66. Diccionario geográfico, estadístico, histórico, de la isla de Cuba. Madrid, Impr. del estab. de Mellado, 4 v.  
1868-78. Historia de la isla de Cuba. Madrid, C. Bailly-Bailliére; Nueva-York, Bailliére hermanos, 4 v.

PINKERTON, JOHN

- 1808-14. A general collection of the best and most interesting voyages and travels in all parts of the world . . . . London, Longman, Hurst, Rees, and Orme, 17 v.

PISO, WILLEM

1648. Historia natvralis Brasiliae . . . . Lvgdvnvm Batavorvm, apud F. Hackium, et Amstelodami, apud L. Elzevirium, 2 v. in 1.

POP, G. F.

1867. De geneeskunde bij het Nederlandsche zeewezen. *Geneeskundig tijdschrift voor de zeemagt*, 5: 103-138.

PRESCOTT, WILLIAM HICKLING

1873. History of the conquest of Mexico . . . . Ed. by John Foster Kirk. Philadelphia, J. B. Lippincott & co., 3 v.

PRÉVOST, ANTOINE FRANÇOIS, *called* Prévost d'Exiles

- 1747-80. Histoire générale des voyages . . . . La Haye, P. de Hondt, 25 v.

PRINGLE, *Sir* JOHN

1810. Observations on the diseases of the army. New ed. London, Printed for J. J. Stockdale.

PUGNET, JEAN FRANÇOIS XAVIER

1804. Mémoires sur les fièvres de mauvais caractère du Levant et des Antilles . . . . Lyon, Reymann.

PURCHAS, SAMUEL

1625. Purchas his pilgrimes . . . . London, Printed by W. Stansby for H. Fetherstone, 4 v.

## PYM, Sir WILLIAM

1848. Observations upon Bulam, vomito-negro, or yellow fever, with a review of "A report upon the diseases of the African coast, by Sir William Burnett, and Dr. Bryson," proving its highly contagious powers. London, J. Churchill.

## RAMUSIO, GIOVANNI BATTISTA, 1485-1557.

- 1563-83. . . . Navigazioni et viaggi. 3. ed. Venetia, Nella stamperia de Givnti, 3 v.

## RAND, ISAAC

1800. Of the epidemic lately prevalent in Boston. *Med. repository*, New York, vol. II, 1798-99, 2d ed. 1800, p. 442-455.

## REED, WALTER, AND CARROLL, JAMES

1901. The prevention of yellow fever. *Med. record*, 60: 641-649.

## REED, WALTER, AND CARROLL, JAMES, AND AGRAMONTE, ARISTIDES

1901. Experimental yellow fever. *Trans. Assn. of Am. phys.*, Philadelphia, 16: 45-70, 71.

## RESENDE, GARCIA DE, 1470-1536.

- Chronica de el-rei D. João II. Lisboa, Escriptorio, 147 rua dos Retozeiros, 1902, 3 v.

## REY, H.

1878. Contributions à la géographie médicale: L'île de Fernando-Po. *Arch. de méd. navale*, 29: 401-409.

## ROBELO, CECILIO AUGUSTIN

1888. Vocabulario comparativo castellano y nahuatl . . . Cuernavaca, L. G. Miranda, impresor.

## ROBERT, LOUIS JOSEPH MARIE

1826. Guide sanitaire des gouvernemens européens, ou, Nouvelles recherches sur la fièvre jaune et le choléra-morbus . . . Paris, Crevot, 2 v.

## ROCHA PITTA, SEBASTIÃO DA

1730. Historia da America portugueza . . . Lisboa Occidental, J. A. da Sylva.

## ROSENAU, M. J., AND GOLDBERGER, JOSEPH

1906. The hereditary transmission of the yellow fever parasite in the mosquito. (In U. S. Pub. health and mar.-hosp. service. Yellow fever institute, bull. no. 15. Washington, Govt. print. off.)

## ROSS, Sir RONALD

1911. The prevention of malaria. 2d ed. London, J. Murray.

## RUFZ DE LAVISON, ÉTIENNE

1869. Chronologie des maladies de la ville de Saint-Pierre (Martinique) depuis l'année 1837 jusqu'à l'année 1856. Paris, J. B. Baillière et fils.

## RUSH, BENJAMIN

1796. An account of the bilious remitting yellow fever, as it appeared in the city of Philadelphia, in the year 1793. Edinburgh, J. Symington.
1799. A second address to the citizens of Philadelphia, containing additional proofs of the domestic origin of the malignant bilious, or yellow fever . . . Philadelphia, Printed by Budd and Bartram for T. Dobson.
1803. Facts intended to prove the yellow fever not to be contagious, and instances of its supposed contagion explained upon other principles. *Med. repository*, 6: 135-150.



1805. An inquiry into the various sources of the usual forms of summer & autumnal disease in the United States . . . . To which are added, facts, intended to prove the yellow fever not to be contagious. Philadelphia, J. Conrad & co.
- SACO, JOSÉ ANTONIO  
1879. Historia de la esclavitud de la raza africana . . . . tomo 1. Barcelona, J. Jepús.
- SAHAGÚN, BERNARDINO DE, *d.* 1590.  
Historia general de las cosas de Nueva España . . . . Dala á luz con notas y suplementos Carlos María de Bustamante . . . México, Impr. . . . A. Valdés, 1829-30, 3 v.  
Historia de la conquista de México . . . . Pub. por separado de sus demás obras C. M. de Bustamante . . . México, Impr. de Galvan à cargo de M. Arévalo, 1829.  
Histoire générale des choses de la Nouvelle-Espagne, traduite et annotée par D. Jourdanet et par Rémi Siméon . . . . Paris, G. Masson, 1880.
- SAN BUENAVENTURA, GABRIEL DE  
1684. Arte de la lengua maya. Mexico, Por la viuda de Bernardo Calderon.
- SAN THOMÉ AND PRINCIPE  
*See* Great Britain, *Foreign Office. Historical Section.*
- SCHOTTE, JOH. PETER  
1782. A treatise on the *synochus atrabiliosa*, a contagious fever, which raged at Senegal in the year 1778 . . . . London, J. Murray.
- SCOTT, JOHN, *fl.* 1634-1696.  
The description of Barbados. Sloane 3662—British museum. By John Scott, geographer to King Charles II. [London? 1907]  
Repr. from *The Weekly argosy* of 17th August, 1907.
- SEIDELIN, HARALD, AND SUMMERS-CONNALL, SOPHIA  
1914. Notes upon the biology of *Stegomyia fasciata*. *Yellow fever bureau bull.*, Liverpool, 3: 187-192.
- SIGAUD, JOSEPH FRANÇOIS XAVIER  
1844. Du climat et des maladies du Brésil . . . . Paris, Fortin, Masson et c<sup>ie</sup>.
- SMITH, DAVID  
1844. Remarks on the epidemic fever of 1843. *Edinburgh med. & surg. jour.*, 62: 62-81.
- SOUTHEY, ROBERT  
1817-22. History of Brazil . . . . London, Longman, Hurst, Rees, Orme, and Brown, 3 v.
- STANLEY, Sir HENRY MORTON  
1885. The Congo and the founding of its free state . . . . New York, Harper & brothers, 2 v.
- STEDMAN, GEORGE W.  
1828. Some account of an anomalous disease which raged in the islands of St. Thomas and Santa Cruz, in the West Indies . . . . 1827-28. *Edinburgh med. & surg. jour.*, 30: 227-248.
- STOKES, WILLIAM  
1876. Lectures on fever: delivered in the theatre of the Meath hospital and county of Dublin infirmary. Philadelphia, H. C. Lea.

## TERNAUX-COMPANS, HENRI

1840. Pièces sur le Mexique. Inédites. Paris, A. Bertrand. (Voyages, relations et mémoires originaux pour servir à l'histoire de la découverte de l'Amérique . . . . t. 16)

## TODD, JOHN LAUNCELOT

1922. The relapsing fevers. (*In* The practice of medicine in the tropics by many authorities, ed. by W. Byam and R. G. Archibald, vol. II, pp. 1254-1277. London, H. Frowde and Hodder & Stoughton)

## TORQUEMADA, JUAN DE, fl. 1600.

- Primera [segunda, tercera] parte de los veinte i vn libros rituales i monarchia indiana . . . . Madrid, N. Rodriguez Franco, 1723, 3 v.

## TOWNE, RICHARD

1726. A treatise of the diseases most frequent in the West-Indies, and herein more particularly of those which occur in Barbadoes . . . . London, Printed for J. Clarke.

## TOZZER, ALFRED MARSTON

1921. A Maya grammar, with bibliography and appraisalment of the works noted. Cambridge, Mass., The Museum. (Papers of the Peabody museum of American archaeology and ethnology, Harvard University, vol. ix)

## TYLER, LYON GARDINER

1900. The cradle of the republic: Jamestown and James River. Richmond, Va., Whittet & Shepperson.  
1906. ———. 2d ed. Richmond, The Hermitage press, inc.

## ULLOA, ANTONIO DE

1748. Relacion historica del viage a la America Meridional . . . . Madrid, A. Marin, 4 v. in 2.  
1772. Noticias americanas: entretenimientos phisicos-historicos, sobre la América Meridional, y la Septentrional [!] oriental . . . . Madrid, F. Manuel de Mena.

## UNANUE, JOSÉ HIPÓLITO

1815. Observaciones sobre el clima de Lima, y sus influencias en los seres organizados, en especial el hombre . . . . 2. ed. Madrid, Impr. de Sancha.

## U. S. PUBLIC HEALTH SERVICE

1912. Annual report of the surgeon-general for the fiscal year 1912. Washington, Govt. print. off.

## VILLALBA, JOAQUIN DE

1803. Epidemiologia española, ó, Historia cronológica de las pestes, contagios, epidemias y epizootias. . . . Madrid, En la imprenta de D. Fermin Villalpando, 2 v. in 1.

VILLAULT, NICOLAS, *sieur de Bellefond*

1669. Relation des costes d'Afrique, appellées Guinée . . . . Paris, D. Thierry.

## VITET, LUDOVIC

1844. Histoire de Dieppe. Paris, C. Gosselin.



WAGENAAR, JAN

1782. *Vaderlandsche historie . . . . der nu Vereenigde Nederlanden . . .*  
Amsterdam, P. Schouten, 21 v.

WALCKENAER, CHARLES ATHANASE, *baron*

1842. *Collection des relations de voyages, par mer et par terre, en différentes parties del'Afrique, depuis 1400 jusqu'à nos jours.* Paris, Chez l'éditeur, 21 v.

WALSH, E.

1806. An account of a malignant fever, which appeared in the garrison of Quebec during the autumn of 1805 . . . . *Med. & phys. jour.*, London, 15: 446-455.

WARREN, HENRY

1740. A treatise concerning the malignant fever in Barbados and the neighboring islands; with an account of the seasons there, from the year 1734 to 1738. In a letter to Dr. Mead. London, F. Giles.

WEBSTER, NOAH

1799. A brief history of epidemic and pestilential diseases . . . . Hartford, Printed by Hudson & Goodwin, 2 v.

YELLOW FEVER: A COMPILATION OF VARIOUS PUBLICATIONS.

1911. Results of the work of Maj. Walter Reed, Medical corps, United States Army, and the Yellow fever commission . . . . Washington, Govt. print. off., 1911. (U. S. 61st Cong. 3d sess. Senate Doc. 822)

YELLOW FEVER COMMISSION (WEST AFRICA)

1915. Reports on questions connected with the investigations of non-malarial fevers in West Africa. *Yellow fever bureau bull.*, Liverpool, Suppl. to vol. 2.
1916. Fourth and final report. London, J. & A. Churchill.





## INDEX TO AUTHORS

- Acosta, 93  
Agramonte, Aristides, 4, 9  
Alcala, Ermilio Solis, 134  
Andral, 61  
Anghierra, Peter Martyr d', 204  
Anson, 63  
Anstie, Francis Edmund, 60  
Aragão, H. deB., 7  
Arejula, Juan Manuel de, 56, 250  
Arrott, James, 59, 107  
Asensio y Toledo, José Maria, 145  
Asseline, David, 210  
Audouard, Mathieu François Maxence, 58, 67  
Augustín, George, 54, 61  
Avreu, Rodriguez de, 220  
Azurara. *See* Eannes de Azurara, Gomes.
- Bacot, A., 16  
Baerle, Kaspar van, 238  
Balfour, Andrew, 72, 75  
Ballot, Victor Alexic Louis François, 65  
Bancroft, Edward Nathaniel, 52  
Barbot, Jean, 222, 225, 226, 227, 229, 230, 245, 246, 248, 249  
Barros, João de, 63, 203, 212, 216, 219  
Bauer, J. H., 5, 7, 8  
Beato, 191  
Beazley, Charles Raymond, 210  
Beeuwkes, H., 8  
Benzoni, Girolamo, 124, 133  
Bérenger-Féraud, Laurent Jean Baptiste, vii, 45, 62, 69, 137, 154, 159, 175, 192, 194, 195, 218, 233, 238, 239, 243, 245, 261, 262, 263, 268  
Bernáldez, Andrés, 216  
Beyer, G. E., 13  
Biondelli, Bernardino, 107  
Blane, Gilbert, 59  
Bouton, Jacques, 78, 180  
Bor, Pieter Christiaanszoon, 251, 252  
Boyce, Rubert William, 13  
Bravo, Francisco, 61  
Breton, Raymond, 68, 146, 180, 181, 182, 183, 184, 185, 186  
Brinton, Daniel Garrison, 89, 100, 116, 117, 119, 120, 121, 122, 123, 124, 127, 129, 131, 132, 146  
Brito Friere, Francisco de, 240  
Brown, Grace, viii  
Bruce, Alexander, 63, 250, 261  
Brumpt, Émile, 58  
Buchanan, George, 43  
Bunbury, Edward Herbert, 209  
Burnett, William, 70  
Busto y Blanco, Fernan del, 65, 243
- Carillo y Ancona, Crescencio, 124, 125, 126, 127, 128, 129, 131  
Carr, G. Jameson, viii, 10, 12  
Carroll, James, 4, 9, 15, 16  
Carter, Henry Rose, 4, 5, 7, 9, 12, 17, 18, 20, 38, 134, 193, 251, 252  
Casas, Bartolomé de las, 114, 155, 156, 157, 159, 160, 162, 164, 165, 166, 167, 168, 172, 173, 174, 175, 177, 179, 204  
Cavassa, Nicolas E., 15  
Chaillé, Stanford Emerson, 45, 180, 187  
Chaillon, 25, 26  
Chanca, 49, 62, 155, 156  
Chimalpahin Quauhtlehuanitzin, Domingo Francisco de San Anton Muñon, 101, 102, 103  
Chirac, Pierre, 58  
Chisholm, Colin, 66, 67, 70  
Christophers, S. R., 40  
Churchill, Awnsham, 217  
Churchill, John, 217  
Clarke, James Stanier, 217  
Cocom, Juan, 132  
Cogolludo. *See* López de Cogolludo, Diego  
Connor, Michael E., viii, 14, 19, 21, 109

- Cordeiro, Luciano, 219  
 Cormack, John Rose, 59, 107  
 Cornilliac, Jean Jacques, 39, 59, 74, 154, 185, 264  
 Corre, Armand Marie, 65, 264  
 Cragg, Francis William, 58  
 Craigie, David, 59, 107  
  
 Dapper, Olfert, 222, 224, 226, 227, 229, 230, 245, 248  
 Dampier, William, 166  
 Davila, Pedrarias, 94  
 Davis, C. E., 42  
 Davis, N. C., 7  
 d'Anghierra. *See* Anghierra, Peter Martyr d'  
 Del Monte. *See* Monte y Tejada, Antonio del.  
 Desgenettes, René Nicolas Dufriche, 70  
 de Witte, Nicolas, 92, 112  
 Diaz del Castillo, Bernal, 68, 94, 96, 101, 104, 105, 106, 109, 114, 115, 136, 141, 142, 143, 162  
 Drake, Daniel, 70  
 Dunn, Lawrence H., 32  
 Duran, Diego, 92, 93, 112  
 Du Tertre, Jean Baptiste, 51, 63, 64, 68, 77, 84, 146, 150, 153, 180, 181, 182, 183, 185, 186, 194, 195  
 Dutroulau, Auguste Frédéric, 50, 65, 264  
 Dyar, Harrison Gray, 3, 4, 13, 14, 29, 82  
  
 Eannes de Azurara, Gomes, 203, 215  
 Eden, Richard, 217  
 Edwards, F. W., viii, 3, 82  
 Elliott, John, 58  
 Ellis, A. B., 209  
 Espinosa, Alonso de, 54, 241  
 Estancelin, Louis, 210  
  
 Faget, Jean Charles, 68  
 Faria e Sousa, Manuel de, 222  
 Ferguson, William, 69, 70  
 Fergusson, William, 66, 67, 70  
 Ferreira da Rosa, João, 49, 151, 195, 238, 239  
  
 Finlay, Carlos Juan, 15, 28, 44, 54, 74, 88, 113, 125, 127, 129, 131, 137, 147, 154, 156, 157, 158, 159, 175, 179, 187, 188, 190, 192, 268  
 Fiske, John, 75, 76  
 Flu, Paul Christian, 16, 85  
 Fowler, C. E. P., 72  
 Francis, Edward, 11, 16  
 Frobisher, M., 8  
  
 Gaffarel, Paul Louis Jacques, 210  
 Gamble, 68  
 García Icazbalceta, Joaquín, 93, 100  
 Göldi, Emil August, 83  
 Goldberger, Joseph, 4  
 Gomara, 93, 94, 114, 115  
 Gordon, R. M., 4, 122, 125, 129, 130, 131  
 Gorgas, William Crawford, 18  
 Gouzien, 58, 199  
 Graves, Robert James, 51, 59, 107  
 Guiteras, Juan, 21, 22, 23, 45, 52, 136, 193  
 Guyon, Jean Louis Geneviève, 220  
  
 Hakluyt, Richard, 217, 218, 234, 243, 245, 251  
 Hanson, Henry, viii, 11, 15  
 Hawkins, Richard, 233  
 Hernandez, Juan Martinez, viii, 120, 121, 126, 127, 128, 129, 130  
 Herrera y Tordesillas, Antonio de, 84, 94, 96, 110, 111, 112, 113, 155, 157, 159, 160, 164, 165, 167, 168, 172, 173, 177, 178, 203, 204, 267  
 Hillary, William, 68  
 Hindle, Edward, 7  
 Hirsch, August, 262  
 Hoil, Juan Josef, 131  
 Howard, Leland Ossian, viii, 4, 13, 14, 29, 82  
 Hudson, N. P., 5, 7, 8  
 Hughes, Griffith, 56, 68, 151, 197, 261  
 Humboldt, Alexander von, 32, 62, 106  
  
 Icazbalceta. *See* García Icazbalceta, Joaquín.  
 Irving, Washington, 74



- Johnson, James, 52  
 Jones, James, 45  
 Jourdanet, 100, 101, 106, 112  
  
 Kitchen, S. F., 8  
 Knab, F., 4, 13, 14, 29, 82  
  
 La Roche, René, 58, 59, 61, 68  
 Labat, Jean Baptiste, 51, 73, 107, 133, 196, 210, 222, 226, 228, 229, 230, 245, 246  
 Laet, Joannes de, 240  
 Laigret, V., 8  
 Lancisi, 256  
 Landa, Diego de, 93, 117, 121, 123, 124, 132, 136  
 Larrey, Dominique Jean, 70  
 Las Casas. *See* Casas, Bartolomé de las.  
 Lassis, 61  
 Lavison, Rufz de, 65  
 LePrince, J. A., 13, 15  
 Leblond, Jean Baptiste, 55  
 Lepoix, 239  
 Liebermeister, 45  
 Ligon, Richard, 56, 146, 149, 152, 153  
 Lind, James, 58, 84, 206, 245, 254, 255, 256  
 Linnaeus, 3  
 Lloyd, W., 8  
 López de Cogolludo, Diego, 52, 84, 138, 146, 147, 148, 151, 152, 254  
 Lopez, Fernando, 14, 28  
 Louis, 59  
  
 MacGregor, Malcolm Evan, 11, 16  
 McAuliffe, 59  
 M'Kinlay, William, 65, 239  
 McNutt, 106  
 Macfie, John William Scott, 4, 15  
 Magendie, François, 61  
 Mahaffy, A. F., 8  
 Marchoux, É., 4, 14, 15, 16, 17, 264  
 Marmol, 202  
 Martel, Charles, viii  
 Masdevall, Joseph, 61  
 Mathis, C., 8  
 Mélier, François, 25, 26, 35, 43, 85  
  
 Mendieta, Gerónimo de, 61, 92, 93, 100, 101, 102, 103, 104, 107  
 Méricourt, Le Roy de, 65  
 Merriam, Clinton Hart, 28, 29, 30  
 Meteren, Emanuel van, 251, 252, 253  
 Molina Solis, Juan Francisco, 93, 107, 138, 139, 146, 148, 189  
 Monchy, Solomon de, 63  
 Monroe, W. M., 19  
 Monte y Tejada, Antonio del, 49, 62, 155, 156, 157  
 Moreau de Jonnés, Alexandre, 39, 69, 70, 74, 154  
 Morley, Sylvanus Griswold, viii, 116, 118, 121, 123, 124, 134  
 Motolinia, Toribio, 93, 100, 101, 112, 143  
  
 Narvaez, 123  
 Navarrete, Martín Fernández de, 156, 159, 162, 173  
 Noc, F., 51, 52, 60, 72  
 Nogue, M., 51, 52, 60, 72  
 Nott, Josiah Clark, 68  
 Núñez de la Peña, Juan, 241  
  
 Obermeier, 59  
 Olmos, Andrés de, 100  
 Orenstein, A. J., 13  
 Orozco, 93  
 Oviedo y Valdés, Gonzalo Fernández de, 68, 74, 89, 94, 95, 138, 139, 159, 160, 164, 167, 168, 172, 174, 176, 177, 178, 179, 204  
  
 Pecho, Francisco, 146  
 Pelleprat, Pierre, 153, 181  
 Peryassú, Antonio Gonçalves, 16  
 Pezuela y Lobo, Jacobo de la, 188, 189, 190, 192  
 Philip, C. B., 8  
 Pinkerton, John, 217  
 Piso, William, 238  
 Pliny, 256  
 Ponce de Leon, Luis, 104, 105  
 Pop, G. F., 243, 244, 245, 247, 248, 249, 250, 252, 253  
 Pothier, O. L., 13  
 Prescott, William Hickling, 106

- Prévost, Antoine François, 217  
 Pringle, John, 63, 250  
 Pugnet, Jean François Xavier, 70  
 Purchas, Samuel, 217  
 Pym, William, 50, 52, 67, 70  
  
 Ramusio, Giovanni Battista, 202, 203, 211, 215, 223, 225  
 Rand, Isaac, 38  
 Reed, Walter, 4, 9, 15, 16, 188  
 Resende, Garcia de, 219  
 Rey, H., 84  
 Robelo, Cecilio Augustin, 107  
 Robert, Louis Joseph Marie, 61  
 Rocha Pitta, Sebastião da, 150, 196, 238, 239  
 Rose, Wickliffe, vii, viii  
 Rosenau, M. J., 4  
 Ross, Ronald, 18  
 Rush, Benjamin, 68  
 Russell, F. F., viii  
  
 Saco, José Antonio, 158, 209, 212, 216  
 Sahagún, Bernardinò de, 92, 93, 98, 101, 106, 107, 112  
 Salimbeni, A., 14, 15, 16, 17, 264  
 San Buenaventura, Gabriel de, 128  
 Sawyer, W. A., 8  
 Scannell, E. J., viii, 195  
 Schotte, Joh. Peter, 69, 84, 206, 228, 236, 254, 257, 261  
 Scott, John, 149, 153  
 Seidelin, Harald, 4  
 Sellards, A. W., 8  
 Shannon, R. C., 7, 8  
 Shortt, H. E., 40  
 Sigaud, Joseph François Xavier, 195, 228  
 Siméon, Rémi, 101, 102, 103, 106, 107  
  
 Simond, P. L., 4, 14, 16, 17, 264  
 Smith, David, 59, 107  
 Southey, Robert, 238, 240  
 Spinden, Herbert J., 120, 126, 133  
 Stanley, Henry Morton, 226  
 Stedman, George W., 64  
 Stevens, J. W. W., 11  
 Stokes, A., 5, 7  
 Stokes, W., 51, 59  
 Summers-Connall, Sophia, 4  
  
 Ternaux-Compans, Henri, 92, 112  
 Theiler, Max, 8  
 Todd, John Launcelot, 58  
 Torquemada, Juan de, 92, 93, 99, 106, 112, 114  
 Torres, 156  
 Towne, Richard, 51, 68, 261  
 Tyler, Lyon Gardiner, 76  
  
 Ulloa, Antonio de, 55, 62, 81, 109, 197  
 Unanue, José Hipólito, 109  
  
 Villalba, Joaquin de, 54, 61, 63, 71, 81, 102, 189  
 Villault, Nicolas, 210  
 Vitet, Ludovic, 210  
  
 Wagenaar, Jan, 252, 253  
 Walckenaer, Charles Athanase, 209, 210, 215, 216, 217, 223  
 Walsh, E., 59  
 Warren, Henry, 51, 52, 56, 57, 68, 125, 261  
 Webster, Noah, 55  
 White, Joseph, H., viii  
  
 Yglesias y Pardo, 84  
 Young, C. J., 4



## INDEX TO SUBJECTS

- "A bicha." *See* "Bicho"  
 Accéso pernicioso, el, 73  
 "Acclimating fevers," 64  
 "Acclimation fever," 222  
 Acclimatization fever, 194  
 Acid, gastric, 50  
 Adriansen, 239  
*Aedes* (*Aëdimorphus*) *apicoannulatus* (Edw.), 6  
*Aedes aegypti*, 5, 6, 9, 10, 11, 24, 27, 28, 29, 32, 33, 34, 36, 37, 40, 43, 62, 65, 66, 85, 88, 96, 100, 116, 137, 142, 143, 144, 147, 148, 152, 199, 203, 218, 220, 232, 239, 240, 250, 255, 266, 269  
*Aedes africanus* (Theo.), 6  
*Aedes calopus* (*aegypti*), 13  
*Aedes* (*Ochlerotatus*) *scapularis*, 6  
*Aedes* (*Ochlerotatus*) *serratus*, 6  
*Aedes simpsoni* (Theo.), 6  
*Aedes* (*Stegomyia*) *aegypti*, 3, 7, 44, 81, 188, 192, 198, 265, 270, 271  
*Aedes* (*Stegomyia*) *alba picta*, 7  
*Aedes* (*Stegomyia*) *luteocephalus* (Newst.), 6  
*Aedes* (*Taeniorhyncus*) *taeniorhyncus*, 6  
*Aedes vittatus* (Bigot), 6  
*Aegypti*, "critical number" of, 18  
*Aegypti* infestation, 28  
*Aegypti* larvae, 12  
*Aegypti*, transporting, 25  
 Africa, 6, 12, 22, 23, 39, 56, 62, 64, 67, 69, 70, 82, 83, 84, 107, 110, 198, 199, 203, 205, 207, 208, 209, 210, 212, 213, 218, 220, 222, 224, 226, 231, 242, 245, 249, 254, 261, 262, 263, 264, 265, 266, 269, 270, 271  
 African slave trade, 212, 231  
 Agua ceniza, 10, 14  
 Aguilar, Gerónimo de, 104, 136  
 Alabama, 44  
 Alajuela, Costa Rica, 32  
 Albermarle, 75, 192  
 Alcala, Ermilio Solis, 134  
 Alcoholism, 51  
 Almendares, Fray Alonso Enriquez de, 191  
 Amaquémécan-Chalco, 103  
 Amazon Valley, 89  
 America, 6, 7, 11, 22, 23, 31, 33, 52, 54, 55, 57, 60, 63, 64, 67, 68, 69, 81, 82, 83, 84, 85, 86, 94, 105, 125, 137, 139, 154, 157, 158, 178, 187, 198, 199, 203, 204, 205, 208, 214, 218, 220, 229, 231, 245, 254, 261, 262, 263, 267, 269, 270, 271  
 American Indian, 81, 270  
 Anales de Chimalpahin, 91, 98  
 Anales de Cuauhtitlan, 91  
 Anales de Tecamachalco, 90, 103, 108  
 Andean Cordilleras, 31  
 Andean plateau, 55, 62, 109  
 Andrada, Gomez Freyre de, 196, 239, 240  
 Anemia, 74, 150, 151, 186  
 Anghierra, Peter Martyr d', 204  
 Angola, 204, 213, 219, 222, 223, 228, 232, 262, 270  
 Animals, susceptible, 5  
 Anne-Marie, 25, 26, 35, 43  
*Anopheles*, 66, 69, 71, 110, 116, 157  
*Anopheles gambiae* (Giles), 6  
 Antarctic Current. *See* Humboldt's Current  
 Antilles, 69, 190, 226, 245  
 Appalachian Mountains, 32  
 Arab traders, 208, 241  
*Arequipa*, 25, 35  
 Arguim, 212, 213, 219  
 Arkansas, 32  
 Arrowack Indians, 89  
 Ascension Island, 71, 263  
 Asensio y Toledo, José María, 145  
 Asia, 38, 64, 266  
 Astrakhan, 102  
*Atelus*, 5

- Atlantic coast, 32  
 Atrato River, 174, 175  
 Australia, 76, 83  
*Avon*, 37  
 "Ayres maléficos," 140  
 "Azafranós," 74  
 Azambuja, Diogo d', 213, 219  
 "Azanegues," 202, 212  
 Azores, 218, 248  
 Aztec Empire, 112, 198  
 Aztec records, 267  
 Aztecs, 53, 116, 117  
 Azurara, 202  
 Azurara's Chronicle, 215  
  
 Bacon, 76  
 Bahia, 11, 150, 192, 196, 205, 207, 238, 239, 240  
 Bakalar, 152  
 Balboa, 159, 162, 164, 174, 175, 176  
 Balearic Islands, 193  
 Baltimore, 28, 43, 44  
 Bangkok, 196  
 Baracoa, 192  
 Barbados, 35, 51, 52, 56, 57, 66, 68, 71, 78, 146, 149, 151, 152, 261  
 Barbary States, 183, 202, 208  
 Barbuda, 185  
 Barranquilla, 20  
 Basse Terre, 153  
 Bastidas, 179  
 Bats, vampire, 138  
 Battimansa, 215  
 Bayamo, 192  
 Bazaine, 65  
 Beato, Dr., 191  
 Belem 169, 171, 172, 173  
 Benin, 217, 218, 231, 233  
 Benin, Bight of, 69  
 Benin, Gulf of, 216  
 Benin River, 217  
 Berbers, 209, 212  
 Beri-beri, 186  
 Béthencourt, Jean de, 241  
 Bezeghichi, King, 212  
 "Bicho," or "bicha," 55, 150, 195, 197, 225, 227, 228  
 "Bichos no Cu," 225, 226  
  
 Bile, black, 51, 260  
 Bile, yellow, 258,  
 Bird, 215, 217, 218  
 Bishopp, 260, 261  
 "Bitios de Cu," 227, 228, 229  
 "Bitios de Ku," 225  
 Blood-vomit. *See* Vomit, blood-  
 Bloody flux, 227, 230, 246  
 Body-louse, 60  
 Bogotá, 204  
 Boston, 38, 43, 44, 71  
 "Botijas," 12  
 Brandenburg, 213  
 Brazil, 6, 10, 12, 14, 23, 65, 150, 194, 195, 196, 210, 227, 230, 238, 239, 252, 264  
 Brebon, Antonio, 195  
 Brest, France, 43, 63  
 Brewster, Elder, 76  
 British Colonial Office, 11, 233  
 British Honduras, 142  
 British Museum, 82  
 British West Africa, 67, 70  
 British West African Yellow Fever Commission, 45  
 Brunswick, Georgia, 27  
 Buboes, 56, 58, 115, 133  
 Bubonic plague, 56  
 Bucaramanga, Colombia, 59  
 Buenos Aires, 33  
 Bulam, 261, 262  
 "Burning ague," 236  
  
 Caballero, Admiral, 115  
 Cabañas, 192  
 Cabo de la Vela, Venezuela, 162  
 Cabo Gracias à Dios, Honduras, 163  
 Cadiz, 71, 81, 189, 197  
 Cairo, Illinois, 32  
 "Calentour," 235  
 "Calentura," 244, 247, 252  
 California, 29  
 Callao, 113  
 Campeche, 20, 45, 116, 118, 142, 147, 149, 152, 190  
 Canary Islands, 85, 108, 193, 211, 213, 220, 230, 241, 242, 243, 244, 245, 247, 248, 251, 252  
 Canterbury, 245



- Caonabo, 157  
 Cape Blanco, 211, 212  
 Cape Bojador, 202, 211  
 Cape Cantin, 209  
 Cape "Lopez Gonsalves," 251  
 Cape Nun, 211  
 Cape of Good Hope, 63, 209, 211  
 "Cape of Storms," 211  
 Cape Palmas, 202  
 Cape Verde, 84, 202, 211  
 Cape Verde Islands, 196, 211, 214, 216,  
     220, 224, 231, 233, 234, 236, 238, 239,  
     240, 242, 251, 263  
 Caramancas, King, 219  
 Carbondale, Illinois, 15  
*Cardinal*, 63  
 Caribbean, 21, 22, 39, 85, 115, 137, 141,  
     144, 159, 173, 194, 196, 207, 269  
 Carib-French dictionary, 180  
 Carib Indians, 85, 88, 89, 141, 154, 155,  
     157, 163, 166, 173, 174, 179, 182, 184,  
     185, 188, 267  
 Carib Islands, 155  
 Carleill, 233  
 Cartagena, 20, 44, 71, 164, 189, 197, 234,  
     237, 238  
 Cartagena de las Indias, 81  
 Cartago, 32  
 Carthage, 209  
 Case mortality, malaria and yellow  
     fever, 72  
 Cassia, 226, 227  
 Castile, 74, 108, 136, 137, 177  
*Castilla*, 37  
 Catalans, 211  
 Catalonia, 61  
 Catarrhs, 145  
 Catarro, El, 54  
 Caux, Sieur de, 241  
*Cebus*, 5  
 Celsus, 261  
 Cemaco, 174  
 Cempoalla, 114  
 Central America, 12, 29, 123, 140, 142,  
     143  
 Cenú River, 179  
 Cerro de Pasco, 62, 109  
 Chac Xulub Chen, Chronicle of, 124  
 Chagres River, 89, 168, 173  
 Chaillon, Dr., 25, 26  
 Chalco, 101  
 Champoton, 152  
 Characteristics of yellow fever, 50  
 Charles V, 84, 104, 113  
 Chatham, Earl of, 66, 70  
 Chelsea (Boston), 71  
 Chibcha Indians, 89  
 Chichemecs, 91  
 Chichen Itza, 116, 127, 128, 129, 134,  
     137, 138  
 Chicoaque, Province of, 92  
 Chigoes, 183  
 Chilan Balam, Books of, 119, 120, 121,  
     122, 124, 125, 126, 129, 132, 134,  
     146, 149  
 Chile, 32, 62  
 Chimpanzees, 5  
 Chimu civilization, 85  
*Chippewa*, 37  
 Cholula, 101  
 Chumayel, Chronicles of, 119, 120, 121,  
     122, 123, 124, 125, 129, 130, 131, 149  
 Cisalpinus, 261  
 Coatzacoalco, 110  
 Coatzacoalcos River, 136  
 Cacalchen, 146  
 "Cocoliztle," 103, 106, 111, 112, 113,  
     124, 267  
 Cocom, Juan, 132  
 Cocomes, family of, 132  
 Codex Aubin, 53, 98  
 Codex Rios (Vaticanus), 97, 98  
 Codex Telleriano-Remensis, 53, 91, 97,  
     98, 99, 101, 102, 107, 133  
 Colmenares, Rodrigo de, 163, 164, 172,  
     173, 174  
 Colombia, 12, 59, 89, 159, 162  
 Colon, 12, 71  
 Columbus, Christopher, 49, 62, 84, 85,  
     136, 155, 157, 160, 162, 163, 168, 172,  
     204, 210, 241, 269  
 Columbus, Bartolomé, 136  
 Columbus, Diego, 163  
 Commercial voyages, 216  
 Conchillos, Lope, 161  
 Congo, 69

- Congo River, 199, 202  
 "Contágio, El," 49, 69  
 Convalescence, Yellow fever, 50, 74  
 Convulsions, 73  
 Cordilleras, Peru, 32  
 Cordoba, 136  
 Cordova, Gonsalvo de, 160  
 Coro, Venezuela, 21, 22  
 Cortez, Hernando, 53, 90, 94, 101, 104,  
     105, 106, 109, 114, 115, 136, 139, 141  
 Coruña, 244  
 Cosa, Juan de la, 163, 164, 165, 166, 179  
 Costa Rica, 32  
 Cotlaxtla, Province of, 92  
 Council of the Indies, 161  
 "Coup de barre," 64, 68, 78, 146, 180,  
     181, 183, 184, 185, 186, 268, 269  
 Cozumel, 123, 136, 138, 139  
 Cuanaja, 136  
 Cuba, 44, 75, 88, 89, 107, 114, 115, 136,  
     137, 140, 142, 158, 167, 177, 187, 188,  
     189, 190, 192, 269  
 Cúcuta, 20, 222, 223  
 Cueto, 168, 169, 170, 171, 172  
 Culebra District, 178  
*Culex icteroides*, 44  
 Cuna Indians, 89  
*Curlew*, 37  
 Cuzco, 109  
 Cyprus, 61
- Damietta, 70  
 Danske, Holgar, 129  
 Darien, 88, 94, 136, 159, 161, 172, 173,  
     174, 175, 176, 178, 179  
 Davila, Pedrarias. *See* Pedrarias  
 Davis, C. E., 42  
*Deinocerites caneronum*, 12  
 d'Enambuc, 77, 181  
 Dengue, 64, 65, 66, 228  
 du Plessis, 181  
 de Witte, Fray Nicolas, 92, 112  
 Diagnosis, 260  
 Diarrhea, 107, 182, 258, 259  
 Diaz, Bartolomé, 211  
 Diaz, Dinis, 211  
 Dieppe, 77, 181, 183, 210  
 "Dieppois," 210
- Differentiation, yellow fever and ma-  
     laria, 71  
 Diphtheria, 100, 102  
 Disease of collections of men, 50  
 Diseases confused with yellow fever, 49  
 Dispersion, 34, 36  
 Distribution of *Aedes aegypti* and yel-  
     low fever, 24, 34  
 Dominica, Island of, 185, 234  
 Don Alonzo, King, 216  
 Don Juan of Austria, 160  
 Dracaena, 12  
 Drake, Sir Francis, 76, 233, 234, 239,  
     244, 247, 251, 270  
 Dublin, 59, 104, 107  
 Dundee, 59, 60, 104, 107  
 Duration of immunity, 7, 19  
 Dysentery, 55, 149, 182, 195, 224, 228,  
     229, 235, 249
- Eannes, Gil, 211  
 East Africa, 12, 208, 266  
 Ecchymoses, 228, 251  
 Edinburgh, 59, 107, 234  
 Egypt, 99, 208  
 Elimination of yellow fever, 18  
 Elizabeth, Queen, 234  
 Elmina, 84, 204, 213, 216, 219, 231  
 "Embarras gastrique," 65  
 Enciso, 163, 164, 174, 179  
 Endemic areas, 40, 44, 45, 81  
 Endemic foci, 81, 116, 140, 143, 158, 205  
 Endemicity, 20, 21, 81, 199, 267  
 "Enfermedad, La, 56, 69, 74  
 England, 16, 54, 75, 76, 213, 218, 233,  
     235, 256  
 Enriquez, Viceroy, 113  
 "Epidémia, La," 49, 56, 106  
 "Epidémia da bicha." *See* "Bicho" or  
     "bicha"  
 Epidemic areas, 44  
 Epidemics, 27, 39, 41, 43, 44, 49, 50, 54,  
     55, 57, 58, 59, 61, 62, 64, 65, 66, 68,  
     70, 71, 74, 81, 85, 94, 100, 101, 102, 103,  
     104, 106, 108, 109, 110, 111, 130, 140,  
     145, 146, 147, 148, 152, 156, 153, 178,  
     188, 189, 190, 191, 195, 196, 197, 199,  
     205, 206, 221, 229, 233, 241, 243, 244,  
     257, 261, 264, 268, 269



- Epidemiology, 3-45, 54, 71, 148, 263  
*Eretmopodites chrysogaster* (Gra-  
 ham), 6  
 Ericson, Lief, 208  
 Escobar, Pedro, 216, 219  
 Escudo Island, 159, 170  
 Esquivel, 163  
 Essequibo River, 89  
 Ethiopia, 208  
 Eudoxus of Cyzicus, 208  
 Europe, 54, 56, 60, 62, 66, 67, 76, 81, 84,  
 102, 107, 209, 210, 226, 232, 252  
 Evidence, biological, of place of origin,  
 81, 270, 271  
 Evidence, historical, of place of origin,  
 83, 271  
 Expeditions, 136, 138, 142, 158, 160, 162,  
 168, 195, 212, 215, 217, 238, 240, 241,  
 270  
 Experimental studies, 5  
 Extrinsic incubation, 3, 148  
  
 Famine, 182, 183  
 "Famine fever," 58  
 "Febris pulicaris," 236  
 Fer-de-lance, 183  
 Fernandina, Florida, 27, 144  
 Fernando Po, 84  
 "Fever of Bulam," 39, 66, 70, 84, 88,  
 203, 262  
 "Fever of Olinda," 49  
 "Fièvre bilieuse inflammatoire," 192,  
 229  
 "Fièvre inflammatoire," 65  
 "Fièvre pestilentielle," 69  
 "Fièvre verdâtre," 102  
 Florida, 27, 76, 197  
 Fogs, 227  
 "Fortunate Islands." *See* Canary Is-  
 lands  
 Fracastorius, 223, 224  
 France, 43, 54, 77, 181, 185, 186, 213  
 Franciscans, 93, 152, 167  
 French Antilles, 22, 58, 65, 68, 78, 84,  
 125, 153, 175, 192, 198, 229, 232, 261  
 French-Carib dictionary, 180  
  
 Gama, Vasco da, 63, 156, 211  
 Gambia, 199, 206, 211, 254, 263  
 Gambia River, 211, 215, 254  
 Gangrene, 70, 228, 260  
 Garcia, Bartolomé, 114  
 "Garrotillo," 102  
 Gebransten, 243, 251  
 Geographic distribution of *Aedes*  
*aegypti*, 24, 31, 34  
 Georgia, 27, 32  
 Germany, 54  
 Gibraltar, 59  
 "Giraffe, La," 65  
 Glasgow, 104  
 Gold Coast, 11, 23, 209  
 Gomara, 94, 243, 247, 251  
 Gonçales, Antam, 212  
 Goode, Col., 76  
 Gorée, 257  
 Gorgas, William Crawford, 192, 194  
 Grain Coast, 210  
 Granada, 54, 61  
 Gran Canaria, 242, 243, 247, 248, 251  
 "Gras fondu," 246  
 Great Antilles, 188  
 Great Britain, 67, 70, 263  
 Great Desert, 208, 209  
 Greater Antilles, 158  
 Greece, 72  
 Grenada, 39, 66, 84, 262  
 Grijalva, 136  
 Grippe, 55  
 Guadalupe, Peru, 15  
 Guadeloupe, 51, 64, 65, 68, 77, 78, 83,  
 146, 150, 153, 180, 181, 183, 184, 185,  
 268  
 Guanches, 54, 71, 241  
 Guastiques, 100  
 Guatemala, 113, 116, 140, 142, 150, 204  
 Guayaquil, 12, 20, 45  
 Guazacoalco, 110  
 Guerra, 179  
 Guerra, Christobal de, 165  
 Guerrero, Gonzalo de, 136  
 Guianas, 12, 39, 153, 180  
 Guinea, 202, 210, 217, 218, 233, 252, 262,  
 270  
 Guinea Coast, 84, 213, 232, 242, 256

- Guinea, Gulf of, 69, 84, 199, 202  
 Guinea, Lower, 213  
 Guinea, Upper, 202, 209  
 Gulf Quarantine Station, 71  
 Gulstonian Lectures, 52
- Habana. *See* Havana.  
 Habrahan. *See* Wilbraham, Capt.  
 "Hacienda endemicity," 21  
 Halifax, 34, 43  
*Hankey*, 262  
 Hanno, the Carthaginian, 209  
 Harvard University, 120, 126  
 Havana, 4, 11, 20, 21, 22, 25, 27, 37, 39,  
     40, 45, 75, 78, 158, 178, 187, 188, 189,  
     191, 192, 193, 194, 196, 197  
 Hawkins, Richard, 233  
 Hayti, 154, 268  
 Hemorrhages, 50, 56, 58, 63, 73, 107  
 Hendricksz, Baldwin, 240  
 Henry, Prince, 203, 211, 212, 215  
 Heredia, Costa Rica, 32  
 Hernandez, Martinez, 121, 126, 128,  
     129, 130  
 Herodotus, 208  
 Heyn, Admiral Pieter, 238, 239  
 Higuey, Province of, 88, 89, 155  
 Hippocrates, 261  
 Hispaniola, 62, 74, 84, 89, 115, 137, 138,  
     139, 154, 155, 157, 158, 159, 160, 177,  
     204, 234, 237, 267, 268  
 Hoil, Juan Josef, 131  
 Holland, 213, 230, 243, 245, 246, 247, 249,  
     251, 252  
 Honduras, 113, 136, 137, 163  
 Hookworm disease, 186  
*Hound*, 254, 256  
 Huasteca, Province of, 92, 112  
 Huastecs, 92, 116  
 Huexalzinco, 93  
 "Huey çahuatl," 101, 184  
 "Hueyzahuatl," 107  
 Humboldt, 32, 62, 106  
 Humboldt's Current, 31  
 Hydrogen-ion concentration, 10
- Identification of yellow fever, criteria  
     for, 206, 269
- "Iepoulicáatina," 184  
 Illinois, 15, 32  
 Imagoes, 11, 15, 41  
 Immunity to yellow fever, 7, 19, 21, 139,  
     193, 204, 222, 264  
 Immunological identity of viruses, 6  
 Incidence, yellow fever, 72  
 Incubation of yellow fever, 3, 4, 16, 37,  
     148, 255  
 India, 23, 58, 211  
 Indians, 94, 95, 101, 102, 103, 106, 108,  
     109, 111, 136, 138, 140, 141, 145, 146,  
     147, 148, 149, 150, 151, 154, 156, 157,  
     160, 163, 165, 167, 171, 172, 173, 174,  
     175, 176, 179, 182, 188, 204, 205, 270  
 Indians, American, 81, 270  
 Indians, Arrowack, 89  
 Indians, Carib, 85, 88, 89, 141, 154, 155,  
     157, 163, 165, 166, 173, 174, 179, 182,  
     184, 185, 188, 267  
 Indians, Chibcha, 89  
 Indians, Cuna, 89  
 Indians, Lucayan, 89  
 Indians, Mexican, 94  
 Indians, San Blas, 89  
 Indies. *See* West Indies  
 Infectible territory, 35, 39, 40  
 Infection, 6, 21, 264  
 Infective man, 3, 4, 36  
 Infective monkeys, 6  
 Infective mosquitoes, 4, 34  
 Influenza, 54, 55, 72, 191  
 Influenzal pneumonia, 62, 109  
 Insect-borne diseases, 58  
 Insect vector of yellow fever, 3, 45, 270,  
     271  
 Intermittent fever, 183  
 International Health Board, 11, 12, 20,  
     109, 195  
 Isabel, 84, 156, 157, 184  
 Italy, 72, 102, 160  
 Izamal, 121, 147, 148
- Jackson, Jacob, 152  
 Jackson, Mississippi, 41  
 Jacksonville, Florida, 197  
*Jacobine*, 71  
 Jaffa, Syria, 70



- Jalapa, 32  
 "Jaloffes," 216  
 Jamaica, 78, 88, 158, 163, 177, 183  
 Jamestown, 75, 76, 157, 175, 178  
 Jaundice, 50, 56, 58, 60, 61, 62, 74, 132  
 Jaundice, leptospiral, 7  
 Jenné, 202  
 Jesuits, 108, 153, 180  
 Jews, 222  
 Jol, Admiral, 229, 245  
  
 Kennebec, 76  
 Key West, 44  
 Kordofan, 264  
 Kukulcan, 129  
  
 Lagos, 22, 71  
*Lagothrix*, 5  
 Laguna, battle of, 54  
 La Mime, 210  
 Lancisi, 256  
 Lane, 76, 235  
 La Rochelle, 153  
 Larvae, mosquito, 11, 12, 14  
 Las Animas Hospital, 4, 44  
 Las Cascadas, 179  
 Laudonnière, 76, 178  
*Le Boeuf*, 153  
 Le flux de sang," 195  
 Leith, 59  
 Lencero, 115  
*Leptospira icteroides*, 7  
 Leptospiral jaundice, 7  
 Lesser Antilles, 21, 22, 39, 84, 88, 89,  
 154, 155, 183, 188, 262  
 Levant, 56, 64  
 Levantine plague, 56, 68  
 Life-zones, 30  
 Life-zones of *Aedes aegypti*, 28  
 Lima, 55, 109  
 Lincolnshire, 66  
 Linnaeus, 3  
 Lirio, 178  
 Lisbon, 151, 196, 212, 213, 219, 238, 239,  
 240  
 Lok, John, 217, 218  
 London, 217, 261  
 Louisiana, 55, 199  
  
 Louse-borne disease, 58, 236  
 Lower Austral Zone, 29  
 Lucayan Indians, 89  
 Lugo, Alonzo de, 241  
*Lynn*, 56  
  
*Macacus rhesus*, 5  
*Macacus sinicus*, 5  
 Madagascar, 208  
 Madeira, 209  
 Madeira Islands, 211, 214, 219  
 Madrid, 110, 145  
 Maine, 34, 43  
 Maio, 251, 262  
 "Mala pasada del camino, La," 62  
 Malaria, 23, 66, 68, 69, 70, 71, 72, 74,  
 77, 78, 111, 113, 116, 178, 179, 186,  
 191, 197, 205, 206, 218, 220, 227, 233,  
 235, 236, 245, 249, 256, 262, 270  
 "Mal de buas," 109  
 "Mal de Siam," 51, 66, 73, 77, 78, 88,  
 107, 196, 197, 226, 227, 228, 229, 260  
 Malignant fevers, 190  
 "Matlatotonqui," 102  
 Manhattan, 76  
 Mani, 93, 117, 119, 121, 122, 123  
 Manicongo, 252  
 Maracaibo, 20, 44, 222, 223  
 Maracaibo, Gulf of, 89  
 Maranhham, 196, 240  
 Marroquin, Bishop, 113  
 Marseilles, 56  
 Martinique, 51, 56, 57, 64, 65, 77, 78,  
 153, 181, 183, 185, 195, 196, 226, 228,  
 260  
 Mascarenhas, 238, 239  
 Massachusetts, 44  
 Matarap, 161, 165, 168  
 "Matlaltotonqui," 103, 106  
 "Matlazahuatl," 61, 106, 107, 124  
 Maurice, Count, 230  
 Mauritius, 72, 75  
 Maya chronology, 118, 121, 122, 123  
 Maya cimal, 116, 120, 121, 124, 126, 132,  
 133  
 Maya country, 85, 116, 136, 145, 198,  
 203, 207, 267  
 Maya culture, 117

- Maya inscriptions, 117, 267  
 Maya language, 117, 128, 184  
 Maya pestilences, 132  
 Maya psychology, 132  
 Maya records, 93, 116, 117, 118, 119, 123, 124, 129, 130, 131, 132, 133, 146, 203, 205  
 Maya ritual, 130  
 Maya scholars, 117, 127  
 Maya tradition, 123, 124, 132, 133  
 Maya "xekik," 116, 125, 149  
 Maya year, 133  
 "Mayacimil," 122, 123, 132  
 Mayapan, 116, 121, 122, 132  
 Mayas, 53, 88, 112, 140, 141, 142  
*Mayflower*, 76  
 Measles, 53, 54, 101, 103, 107, 145  
 Medellin, 104, 105  
 Mediterranean, 81, 208  
 Memphis, Tennessee, 25, 41, 44, 70, 144  
 Menezes, 238, 239  
 Merida, Yucatan, 21, 120, 124, 126, 147, 148, 149  
*Merlin*, 254, 255, 256  
 Mesopotamia, 40  
 Mexican Indians, 94  
 Mexican Plateau, 61, 111, 140  
 Mexico, 12, 14, 25, 32, 53, 61, 62, 85, 90, 91, 94, 96, 99, 100, 101, 105, 107, 109, 110, 118, 123, 128, 136, 137, 139, 140, 141, 142, 143, 158, 190, 203, 204, 264, 267  
 Mexico City, 14, 28, 61, 90, 94, 100, 104, 105, 108, 109, 114, 136  
 Mexico, Gulf of, 92  
 Mice, 5  
 Mississippi, 25, 35, 41, 148, 250  
 Mississippi River, 25  
 Mississippi Valley, 32  
 Mobile, 11, 16, 41, 44, 68  
 "Modorra, La," 49, 54, 94, 71, 104, 109, 159, 176, 177, 178, 179, 241, 252, 268  
 Molina, 93, 189  
 Moloch, Temple of, 209  
 Monkeys, African, 5  
 Monkeys, *Atelus*, 5  
 Monkeys, Brazilian, 5  
 Monkeys, *Cebus*, 5  
 Monkeys, infective, 6  
 Monkeys, *Lagothrix*, 5  
 Monkeys, *Saimiri*, 5  
 Monroy, 165  
 Montejo, 133, 137, 138, 139, 140, 158  
 Montevideo, 33  
 Montezuma, 92, 111, 112, 116, 143  
 Montgomery, Alabama, 44  
 Montoir, 25, 26  
 Moors, 212  
 "Morbis pulicaris," 258  
 "Mortalidad," 103, 106  
 Mortality, 50, 52, 58, 59, 60, 72, 75, 77, 78, 96, 98, 100, 101, 102, 113, 139, 154, 156, 173, 178, 183, 185, 188, 191, 192, 193, 206, 230, 234, 236, 237, 248, 249, 253, 256, 262, 268  
 Mosquito breeding and breeding places, 10, 12, 14, 15, 21, 24, 27, 33, 43  
 Mosquito Commission of New Orleans, 13  
 Mosquitoes, 265  
 Mosquitoes, African, 6  
 Mosquitoes, American, 6  
 Mosquitoes, infected, 34  
 Mossi, 202  
 Mosto, Aluise Ca da, 211, 212, 215, 216, 231  
 Mota, Capt., 252  
 Mozambique, 208  
 Mumps, 100  
 Nahuatl language, 92, 98  
 Nahuatl hieroglyphics, 90  
 Nahuatl picture, 97  
 Nahuatl records, 91, 93, 118, 205  
 Nakuk Pech, 124  
 Naples, 58  
 Narvaez, 53, 101, 115, 123  
 Navarrete, 156, 159, 173  
 Negroes, 6, 50, 52, 53, 59, 60, 69, 72, 73, 81, 84, 116, 150, 158, 202, 212, 216, 224, 233, 253, 264, 270  
 Netscher, 230  
 Newburyport, Massachusetts, 44  
 New England, 54  
 Newfoundland, 211  
 New Orleans, 13, 41, 68, 187, 196



- Newport, 75  
 New Spain, 103, 108, 111, 113, 158, 190  
 Newton, 215, 218  
 New York, 24, 32, 42, 44  
 Nicuesa, Diego de, 158, 159, 161, 162, 163, 164, 166, 168, 169, 170, 172, 174, 176, 268  
 Niger Valley, 202  
 "Noche Triste," 101, 115  
 "Nohkakil," 53, 125  
 Nombre de Dios, 88, 140, 145, 161, 163, 171, 172, 173, 174, 176, 187, 234  
 Norfolk, Virginia, 17, 28, 32  
 Normans, 210  
 North Africa, 209  
 North America, 29, 53, 54, 76, 136  
 Norwich, 245  
 Nubia, 208  
 Nuñez, Vasco, 161  
  
 "O bicho." *See* "Bicho"  
 Ocean Springs, Mississippi, 250  
 "Ocnalkuchil," 122, 124, 126, 132  
 Ocoa, 113, 142  
 Ojeda, Alonzo de, 158, 159, 162, 163, 164, 165, 166, 167, 168, 175, 268  
 Olano, Lope de, 168, 169, 170, 171, 173  
 Olinda, 196, 239  
 Olive, de l', 181  
 Ollas, Pueblo de las, 165, 166, 168  
*Oriflamme*, 66, 196, 197, 226, 227  
 Origin of yellow fever, 81, 83, 152, 154, 265  
 Orizaba, 10, 32  
 Ortiz, 114  
 Ortiz, Padre, 104  
 Orwood, Mississippi, 4, 148  
 Ovando, 84, 157  
 Ovid, 67  
 Oviedo, 168, 170  
 Oviposition, 10, 11, 24  
 Ozarks, 32  
  
 Pacho, Francisco, 146  
 Pacific Ocean, 31, 38, 175  
 Paita, 71  
 Palos, 210  
 Paludism. *See* Malaria  
  
 Pamlico, 76  
 Panama Canal, 38  
 Panama, Isthmus of, 20, 21, 22, 159, 160, 174, 194, 204, 234, 267, 268  
 Panama, Republic of, 169  
 Panuco, 92, 96, 110  
 Panuco River, 112  
 Papaloapan River, 21  
 "Paperas," 100, 102, 103  
 Pará, 83  
 Parahyba, Brazil, 10  
 Parasites, 14  
 Paris, 61  
 Parker, William, 152  
 Pasteur Commission, 14, 264  
 Pavoasan, 223, 224, 225  
 Payva, João de, 222  
 Peabody Museum, Harvard University, 120, 126  
 Pechs, 118  
 Pedrarias, 161, 176, 177, 178, 179, 268  
 Pedrarias, Davila, 94  
 Pedrarias the Younger, 179  
 Penã, Nuñez de la, 241  
 Perez, Pio, 133  
 Peritonitis, 51  
 Permanent endemic center, 45  
 Permanent endemic focus, 140, 143  
 Permanent endemic regional focus, 116  
 "Permanent regional focus," 21, 143  
 Pernambuco, 39, 49, 151, 196, 238, 239  
 Peru, 12, 14, 15, 32, 55, 62, 85, 88, 109, 113, 203, 204  
 "Peste," 52, 56, 57, 69, 78, 88, 106, 146, 147, 149, 150, 151, 153, 185, 188, 192  
 "Pestilencia," 81, 106, 192, 224, 233  
 Petechiae, 61, 236, 258, 259, 260  
 Petechial eruptions, 56, 60, 62  
 Petit Dieppe, 210  
 Petit Paris, 210  
 Pharaoh Necho, 208  
 Philadelphia, 24, 28, 32, 42, 43, 44, 68, 76, 120, 125  
 Philip II, 94  
 Phoenicians, 208, 209  
*Phoenix*, 256  
 Pintado, 81, 197, 217  
 Pinzon, Vincente Yañes, 210

- Piso, William, 238  
 Pizarro, 164, 167, 168, 174  
 Plague, bubonic, 56  
 Plague, Levantine, 56, 68  
 Plague, pneumonic, 56, 235  
 Plants, life-zones, 32  
 Playa de Damas, 172  
 "Pleurésie," 102, 103  
 Pleurisy, 102  
 Pliny, 256  
 Plymouth, 76, 77, 178, 217  
 Plymouth colonists, 55  
 Pneumonia, 62, 72, 109  
 Pneumonic plague, 56, 235  
 Pocock, 192  
 Poland, 62  
 Pole, 138, 139  
 "Polka fever," 65  
 Ponce de Leon, Luis, 104, 105  
 Popayan, 62  
 Port Elizabeth, 211  
 Portland, Maine, 34, 43  
 Port Louis, 72  
 Porto Bello, 171, 172, 173, 189, 197  
 Porto Rico, 20, 65, 88, 89, 155, 158, 188  
 Portsmouth, 235  
 Portugal, 212, 213, 216, 222, 236, 242  
 Portuguese discoveries, 211  
 "Portuguese Pilot," 225, 233  
 Portuguese West Africa, 231, 232  
 Potosí, 62  
 Praecordia, 258  
 "Preste Juan de las Yndias, 97, 98  
*Prince Edward*, 35  
 Prince Henry, 203, 211, 212, 215  
 Puerto de Bastimentos, 171, 172  
 Puerto de Misas, 168, 172, 173  
 Puerto Principe, 192  
 Pujadas, Juan, 188, 189, 190  
 Punic inscription, 209  
 Punjab, 58  
 Putrid fever, 188, 230, 246, 260  
  
 Quebec, 43, 59, 60  
 Quemados, 9, 15  
 Quinine, 72, 245  
 Quito, 32, 55, 62, 109  
 Quotidian fever, 68  
  
 Racial resistance to yellow fever, 263, 264  
 Recife, 196, 239  
 Records, 90, 109, 116, 117, 124, 134, 140, 145, 203, 205, 215, 262, 267, 268  
 Red River, 25  
 Red Sea, 40, 208  
 Regional endemicity, 20  
 Regional focus, 21, 140  
 Reinfection of infecting places, 39  
 Relapsing fever, 58, 60, 62, 107, 178  
 Remidios, 192  
*Revenge*, 247  
 Rey, H., 84  
 Rio de Janeiro, 14, 20, 35, 37, 40, 45, 78  
 Rio Grande do Sul, 33  
 Roanoke Island, 235  
 Robelo, Licentiate, 107  
 Rochefort, France, 43, 57  
 Rockefeller Foundation, 5, 12, 21, 22  
 Rocky Mountain fever, 14  
 Rocky Mountains, 29  
*Rosa del Turia*, 84  
  
 Sahara, 202, 209, 212  
 Sailing vessels, 35, 82  
 Saimiri, 5  
 Saint Jago, 234  
 Salamanca, 138, 139  
 Salle, Gadifer de la, 241  
 Saltocan, 94, 109  
 Salt Pond, Gold Coast, West Africa, 11  
 San Blas Indians, 55, 89  
 San Blas lagoon, 166  
 San Buenaventura, Gabriel de, 123  
 Sancti Spiritus, 192  
 Sandoval, 143  
 San José, 32  
 San Juan, 65, 177  
 San Juan de Lua, 113  
 San Juan de Ulua, 104  
 San Juan River, 174  
 San Sebastian, 161, 166, 167  
 San Sebastian, Fort, 246  
 Santa Maria de Redondo, 169  
 Santa-Marta, 165  
 Santarem, Juan, 216, 219  
 Santiago, 75, 192



- Santiago de Cuba, 190  
 Santo Domingo, 20, 74, 88, 89, 104, 139, 155, 158, 159, 160, 163, 164, 167, 171, 175, 187, 188, 191, 241, 244  
 Santos, 33, 35  
 São Jorge da Mina. *See* Elmina  
 São Thiago, 219, 231, 232, 233, 234, 235, 236, 237, 238, 242  
 São Thomé, 195, 205, 211, 214, 219, 220, 222, 223, 224, 225, 227, 228, 229, 230, 233, 243, 245, 247, 248, 249, 250, 251, 256, 260, 270  
 Savannah, Georgia, 27  
 Scarlet fever, 145  
 "Scheurbuick," 244, 248, 250, 251, 252  
 Schistosomiasis mansonii, 195, 228, 229, 249  
 Scurvy, 52, 62, 63, 156, 228, 235, 244, 248, 250, 251  
 Senegal, 52, 60, 69, 72, 199, 206, 211, 212, 228, 254, 260, 262, 263  
 Senegal River, 211  
 Senegambia, 260, 261  
 Serrano, 175  
 Seville, 155, 158  
*S. fasciata*, 83  
*S. fasciata (argenteus: aegypti)*, 82  
 Shafter, Gen., 75  
 Ship Island, Mississippi, 35, 37  
 Siam, 196, 227  
 Sidon, 209  
 Sierra Leone, 12, 22, 199, 202, 209, 216, 262  
 Slave Coast, 39, 84  
 Slave trade, 212, 214, 231, 233  
 Smallpox, 53, 54, 55, 62, 72, 100, 101, 107, 109, 122, 123, 124, 125, 133, 136, 142, 145, 146, 184, 190, 224, 235, 242  
 Smith, John, 75  
 "Society fever," 197  
 Society of Jesus. *See* Jesuits  
 Sociological conditions, 40, 152, 203  
 Soudan, 202, 209  
 South America, 20, 31, 32  
 South Caribbean, 84  
 South Carolina, 32, 199  
 Southern Asia, 38  
 Spain, 54, 61, 72, 74, 102, 104, 105, 106, 110, 137, 138, 140, 142, 155, 156, 157, 159, 160, 163, 177, 193, 236, 241  
 Spanish America, 14  
 Spanish Conquest, 90, 91, 92, 93, 94, 96, 100, 103, 106, 111, 112, 113, 117, 118, 122, 123, 124, 125, 132, 134, 140, 141, 142, 143, 151, 152, 267  
 Spanish Indies, 84  
 Spanish records, 119  
 Spanish writers, 92, 94, 109  
 Spirochetal diseases, 58  
 Spirochetal infections, 72  
*Spironema duttoni*, 58  
*Spironema obermeieri*, 58  
*Spironema recurrentis*, 58  
 Standish, Miles, 76  
 Stanley, Henry M., 226  
 St. Augustin, 108, 112  
 St. Augustine, Florida, 235  
 St. Christophe, 63, 76, 77, 146, 153, 180, 181, 185  
 St. Dominic, 108  
 Steamships, 35  
*Stegomyia*, 3, 12, 13, 16, 82, 271  
 St. Francis, 108  
 St. Francis, Brothers of, 106  
 St. Germaine, 185  
 St. Helena, 263  
 St. Jago, 235  
 St. Jean d'Acre, 56, 70  
 St. Kitts, 77, 153, 181  
 St. Louis de Senegal, 69, 84, 206, 257  
 St. Lucia, 185, 194, 195  
 St. Nazaire, 25, 26, 35, 43  
 Stoddard's Wharf, Boston, 38  
 Storm, Vice-Admiral, 246  
 Storm von Wena, 252  
 St. Pierre de Miquelon, 43  
 St. Sebastian, 166  
 St. Thomas, 20, 64, 223, 225, 244, 245, 248, 251, 252  
 St. Vincent, 185  
 Susceptibility to disease, 78, 151, 185, 263, 270  
 Susceptible animals, 5  
 Susceptible men, to yellow fever, 8, 17, 19, 40, 154, 232, 263, 264, 265, 266

- Swansea, 35, 43  
 Sweden, 213  
 Symptomatology, 73  
 "*Synochus atrabiliosa*," 69, 206, 257, 259  
 Syphilis, 224  
 Syria, 70  
  
 "Tabardete," 61  
 "Tabardillo, El," 61, 62, 100, 102, 103, 104, 145, 146  
*Taeniorhyncus (Mansonoides) africanus* (Theo.), 6  
 Talavera, Bernardino, 167  
 Tampico, 44  
 Taylor, Mississippi, 148  
 Tehuantepec, 92, 117, 141  
 Temperature, 14, 28, 31, 35, 40, 41, 43  
 Teneriffe, 54, 65, 71, 213, 241, 242  
 Tennessee, 41, 44  
 Tenochtitlan, 98  
 "Tepiton çahuatl," 101  
 Tertian fever, 68  
 Texeira, 220  
 Texel, 243  
 Tick-borne infection, 58  
 Tick-borne spirochetoses, 52, 178, 249  
 "Tick fever," 59  
 Tierra caliente, 61, 85, 96, 110, 112, 143, 267  
 Tierra-firme, 88, 158, 159, 161, 162, 164, 178,  
 Tierra fria, 14, 91, 96, 99, 104  
 Timbuktu, 202  
 Tizimin manuscript, 119, 120, 121, 122, 123, 125, 126, 131, 134, 135  
 Tlaltelolco, 101, 108  
 Tlaxcala, 101, 140  
 Tocapilla, Chile, 32  
 Togoland, 85  
 "Tohtomonaliztli," 101, 125  
 Toledo, 160  
 Torre, Count Ramon della, 223  
 Torres, 155, 156, 241  
 Tortuga, 191  
 Totonaca, 100  
 Totopec, 92  
 Towerson, 217, 218  
 Toxins, 264  
  
 "Trancaso, El," 65  
 Transportation of infected men, 36  
 Transportation of infected mosquitoes, 34  
 Trinidad, 158, 192  
 Tristram, Nuño, 212  
 Tropic of Cancer, 247  
 Troy, 147  
 Trujillo, 85, 142  
 Tuaregs. *See* "Azanegues"  
 Tuckey, Capt., 69  
 Tulapan, 118  
 Tumbez, 31  
 Turbaco, 165  
 Typhoid fever, 61, 178  
 Typhus, 50, 54, 56, 58, 60, 61, 62, 104, 105, 107, 109, 145, 178, 235, 236, 242  
 "Typhus icteroides," 60  
  
 Ukraine, 62  
 Ulcers, 78  
 Ulloa, 197  
 Ulua, 136, 142  
 Umbria, Pedro de, 168, 169  
 "Una fiebre pestilencial," 49  
 "Una peste," 49  
 United States, 12, 16, 25, 26, 29, 30, 35, 70, 264  
 United States Public Health Service, 20, 30, 32  
 University of Pennsylvania, 120  
 Upper Guinea, 202, 209  
 Uraba, 163  
 Uraba, Gulf of, 89, 162, 164, 174  
 Urava, 174  
 Urava, Gulf of, 174  
  
 Valderrama, 102  
 Valdivia, 62, 63, 136  
 Valladolid, 138  
 Vallarte, 203  
 Vampire bats, 138  
 Van der Does, Admiral, 229, 230, 241, 243, 244, 245, 246, 247, 251, 252, 253, 270  
 Vasco Nuñez. *See* Balboa  
 Vector, insect, 3  
 Vectors, 3, 6, 45, 60, 62, 81, 266, 270, 271  
 Vega Real, 154, 156, 188, 268



- Velásquez, 187  
 Venesection, 50  
 Venezuela, 21, 22, 162, 191  
 Vera Cruz, 4, 21, 22, 39, 40, 65, 78, 88, 92, 96, 100, 110, 111, 112, 113, 114, 115, 140, 142, 145, 152, 189, 192, 194, 197, 268  
 Veragua, 166, 168, 169  
 Veragua, Province of, 163  
 "Verdoes," 245, 246  
 Vessels, 82, 142, 153, 158, 167, 168, 188, 190, 192, 215, 230, 232, 233, 236, 237, 238, 242, 244, 246, 248, 252, 255, 263, 268  
 Viability of eggs, 41  
 Vibices, 258  
 Virginia, 17, 28, 32, 76, 77, 78  
 Virus, American, 6  
 Virus, West African, 6  
 Vomit, black, 50, 51, 52, 55, 58, 60, 61, 73, 125, 127, 228, 259  
 Vomit, blood, 106, 124, 125, 126, 130, 131, 132, 149, 267  
 Vomiting, 51, 56, 73, 98, 106, 107, 125, 131, 132, 149, 153, 258, 260  
 "Vómito negro," 197  
 "Vómito Negro Fever," 70  
 "Vómito prieto," 52, 197, 259  
 Voyages of the "Dieppois," 210  
  
 Walcheren, 66, 70  
 Warner, 181  
 Wassaw, 209  
*Weasel*, 254, 256  
 West Africa, 6, 7, 11, 16, 22, 23, 45, 70, 81, 82, 83, 84, 198, 199, 200, 203, 205, 208, 209, 210, 211, 212, 213, 214, 215, 216, 217, 218, 220, 231, 239, 242, 249, 254, 262, 264, 266, 269, 270, 271  
 West African negroes, 6  
 West African virus, 6  
 West African Yellow Fever Commission, 5, 12  
 West Indies, 29, 43, 59, 63, 66, 77, 84, 95, 101, 158, 160, 161, 180, 196, 198, 232, 234, 245, 262  
 Whooping cough, 72  
 Wilbraham, Capt., 152  
 Windham, 215, 217  
  
 Windward Islands, 66, 67  
 Witman, Dr., 70  
  
 Xala, 138  
 "Xekik," 124, 125, 126, 130, 132, 133, 149, 267  
 Xelder, Gov., 192  
 Xelofes, 216  
 Xius, 118  
  
 Yaws, 78, 83  
 Yellow fever, Africa, 198, 254, 261  
 Yellow fever, America, 187  
 Yellow fever, biological evidence of place of origin, 81, 265, 270  
 Yellow fever, Brazil, 194  
 Yellow fever, Canary Islands, 241  
 Yellow fever, Cape Verde Islands, 231  
 Yellow fever, causative organism, 5, 6, 8, 34, 38, 40, 178, 199, 265  
 Yellow fever, characteristics, 50  
 Yellow fever, conditions requisite for continued existence, 3  
 Yellow fever, conveyance by vessels, 37  
 Yellow fever, criteria for identification, 206  
 Yellow fever, Cuba, 187, 192  
 Yellow fever, Darien, 174  
 Yellow fever, differentiation between malaria and, 71  
 Yellow fever, diseases confused with, 49-78  
 Yellow fever, dispersion, 24, 34, 36  
 Yellow fever, distinctive characteristics, 50  
 Yellow fever, early history and records, 75, 215  
 Yellow fever, endemicity, 20  
 Yellow fever, general considerations, 81  
 Yellow fever, general postulate, 6  
 Yellow fever, geographical distribution, 24, 34  
 Yellow fever, Guadeloupe, 180  
 Yellow fever, Hayti, 154  
 Yellow fever, historical evidence of place of origin, 83  
 Yellow fever in Caribbean regions, 194  
 Yellow fever in Dutch fleets, 229  
 Yellow fever in early settlements, 219

- Yellow fever, infectible territory, 40
- Yellow fever, Isthmus of Panama, 159, 174
- Yellow fever, Maya country, 116, 136, 145
- Yellow fever, Mexican records, 90
- Yellow fever, Mexico, 110
- Yellow fever, mortality before and after Spanish Conquest, 96, 100
- Yellow fever, mortality among early colonists, 75
- Yellow fever mortality, community, 72
- Yellow fever, non-infectible territory, 40
- Yellow fever, occurrence, 40
- Yellow fever, persistence, 40
- Yellow fever, rate of spread, 38
- Yellow fever, recent experimental studies, 5
- Yellow fever, recognition of, 205
- Yellow fever, regions for examination in America, 85
- Yellow fever regions of importance, 200-201
- Yellow fever, relations of temperature and sociological conditions, 40
- Yellow fever, São Thomé, 222
- Yellow fever, Spanish writers on, 92, 94
- Yellow fever, spontaneous elimination of, 18
- Yellow fever, susceptibility to, 17
- Yellow fever, West Africa, 198, 199, 208, 215, 254
- Yellow fever, Yucatan, 140
- Yellow fever, zones of occurrence, 44
- Yellowness, 73, 151, 259
- "Yetel," 123
- Yol, Admiral, 230
- Ytza, 130
- Yucatan, 14, 20, 21, 45, 52, 84, 85, 92, 93, 98, 116, 120, 123, 125, 126, 132, 133, 136, 137, 140, 141, 142, 143, 144, 145, 147, 148, 149, 150, 151, 152, 178, 180, 188, 190, 192, 196, 198, 204, 205, 207, 220, 254, 261, 265, 267, 268, 269
- Yurbaco, 165
- Zaire River, 69
- Zarco, 220
- Zealand, 66, 70
- Zingara, 71
- Zone, lower austral, 29
- Zones, life, of *Aedes aegypti* 28, 29, 30
- Zone, tropical, 29
- Zumárraga, Archbishop, 93



# *Sans Tache*



## *Sans Tache*

**I**N THE "elder days of art" each artist or craftsman enjoyed the privilege of independent creation. He carried through a process of manufacture from beginning to end. The scribe of the days before the printing press was such a craftsman. So was the printer in the days before the machine process. He stood or fell, as a craftsman, by the merit or demerit of his finished product.

Modern machine production has added much to the worker's productivity and to his material welfare; but it has deprived him of the old creative distinctiveness. His work is merged in the work of the team, and lost sight of as something representing him and his personality.

Many hands and minds contribute to the manufacture of a book, in this day of specialization. There are seven distinct major processes in the making of a book: The type must first be set; by the monotype method, there are two processes, the "keyboarding" of the MS and the casting of the type from the perforated paper rolls thus produced. Formulas and other intricate work must be hand-set; then the whole brought together ("composed") in its true order, made into pages and forms. The results must be checked by proof reading at each stage. Then comes the "make-ready" and press-run and finally the binding into volumes.

All of these processes, except that of binding into cloth or leather covers, are carried on under our roof.



The motto of the Waverly Press is *Sans Tache*. Our ideal is to manufacture books “*without blemish*”—worthy books, worthily printed, with worthy typography—books to which we shall be proud to attach our imprint, made by craftsmen who are willing to accept open responsibility for their work, and who are entitled to credit for creditable performance.

The printing craftsman of today is quite as much a craftsman as his predecessor. There is quite as much discrimination between poor work and good. We are of the opinion that the individuality of the worker should not be wholly lost. The members of our staff who have contributed their skill of hand and brain to this volume are:

*Keyboards:* Minnie Foard, Margaret Foote, Louise Hilpert, Stella Kocent, Viola Schneider.

*Casters:* Charles Aher, Kenneth Brown, George Bullinger, Norwood Eaton, Charles Fick, Henry Lee, Mahlon Robinson, George Smith, Ernest Wann.

*Composing Room:* Arthur Baker, John Crabill, Richard King, Ray Kauffman, Robert Lambert, Emerson Madairy, Theodore Nilson, George Moss, Andrew Rassa, Edward Rice, Henry Sanders, Henry Shea, Vernon Thomas, Austin Uhland, Anthony Wagner, Charles Wyatt.

*Proof Room:* Mary Reed, Alice Reuter, Ethel Ewing, Shirley Seidel, Ruth Jones, Audrey Knight, Ruth Foster, Betty Williams, Viola Woodward, Dorothy Fick, Alice Grabau, Katharine Dudley, Virginia Williams.

*Press:* Herbert Morman, Edward Smith.

*Cutter:* William Armiger.

*Folders:* Laurence Krug, Clifton Hedley.

# *“Sidelights On the Universe”*

SHORT and concise, informative and entertaining volumes; snapshots of science or of scientific problems of current interest. They sell for \$1.00 each.

THE LORE AND LURE OF ARCHAEOLOGY

*Ralph Van D. Magoffin.*

PRACTICAL APPLICATIONS OF HEREDITY

*Paul Popenoe.*

NATURE NARRATIVES

*Austin H. Clark.*

RESEARCH NARRATIVES

Edited by *Alfred D. Flinn.*

(3 volumes, each \$1.00.)

ANIMAL LIFE IN THE CARLSBAD CAVERN

*Vernon Bailey.*

THE BEAVER: ITS WORKS AND ITS WAYS

*Edward R. Warren.*

MAN AND HIS AFFAIRS

*Walter N. Polakov.*

*\$1.00 each*

THE WILLIAMS & WILKINS COMPANY

*Publishers of Scientific Books and Periodicals*

BALTIMORE, U. S. A.





